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SCIENTIFIC PAPERS PRESENTED BEFORE THE AMERICAN GEOPHYSICAL UNION AT ITS SECOND ANNUAL MEETING IN 1921

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These papers were presented at the second annual meeting of the American Geophysical Union held at the National Research Council, Washington, D. C., April 18, 19 and 20, 1921. The names of three of the sections (seismology, volcanology and geophysical chemistry) do not appear in the table of contents, as these meetings were devoted to discussion and no scientific papers were presented.

The American Geophysical Union is the Committee on Geophysics of the National Research Council and is the National Committee for the United States of the International Geodetic and Geophysical Union.

MEASUREMENT OF GRAVITY AT SEA

A Review

By Lyman J. Briggs

METHODS OF DETERMINING GRAVITY AT SEA

The measurement of the acceleration of gravity over the oceans is a matter of interest to the geodesist in the determination of the figure of the geoid and in investigations relating to isostatic compensation. For the requirements of these problems it is desirable that the probable error of the gravity determinations should not exceed one part in 50,000. At first sight this accuracy does not seem to be unattainable, particularly to those who are familiar with the remarkable work that has been done with invariable pendulums on land stations, where the probable error has been reduced to two or three parts in a million. But when we consider that in measurements at sea the vertical acceleration of the ship is imposed upon the gravitational acceleration which we are trying to measure, the difficulty of the problem becomes apparent. For example, in the case of a ship which rises and falls through a height of a meter during a period of 10 seconds, the average vertical acceleration without regard to sign is about 0.004g, or 200 times the permissible probable error of the measurement. Such vertical accelerations of the ship are not uncommon, although the sea is sometimes so smooth that an index point may be set in grazing contact with the surface of a mercury column with almost laboratory precision.

The oscillations of the sensitive element due to the vertical accelerations of the ship may be controlled by damping, but it is doubtful whether the damped system gives the same result as when vertical accelerations are absent. This question cannot be answered from observations at sea, and necessitates laboratory methods of testing gravity apparatus which will be referred to later.

Virtually all the methods that have actually been used for gravity determinations at sea involve and depend upon observations of the length of a mercurial column supported by gas pressure. The pressure p of the gas may be equated to $\rho\ g\ h$ where ρ and h refer to the density and height of the mercury column. Knowing p, ρ and h, the acceleration of gravity g can at once be evaluated. The various methods differ in the manner in which p and h are determined.

Siemens' method: The first actual measurements of the variation of gravity at sea appear to have been made by Sir. Wm. Siemens[1] in 1875. He believed that at sea the value of g was diminished by an amount very nearly proportional to the depth of the ocean, and his primary purpose

[1] C. Wm. Siemens. On determining the depth of the sea without the use of a sounding line. *Phil. Trans.*, 167, 1877, 671-692.

was to develop a sounding apparatus on this principle as an aid to navigation.

His first instrument consisted of a barometer with a large sealed-off air chamber to eliminate the effect of variations in atmospheric pressure (fig. 1). The barometric column included three liquids—mercury, diluted alcohol and juniper oil. The vertical column was expanded into a bulb *b*, at the level where the mercury and alcohol surfaces were in contact, with a second evacuated bulb *d* at the top, which contained the free juniper-oil surface. The readings were made on the position of the alcohol-oil surface *c* in the constricted tube joining the two bulbs. With this device the scale deflection for a given change in *g* was 300 times that of a simple mercurial column.

This instrument proved unsatisfactory and was abandoned in favor of a second "bathometer," which consisted of a steel spring balance of a peculiar type. A vertical steel tube was fitted with reservoirs at top and bottom, the floor of the lower reservoir consisting of a thin corrugated steel diaphragm. This system contained mercury, the free surface of the mercury being in the upper reservoir, which during measurements was open to the atmosphere. The load on the diaphragm was carried by two long steel spiral springs connected to a yoke beneath the diaphragm and suspended from the upper reservoir. The mercury constituted the load on the balance and at the same time served to damp the oscillations through the action of a constriction in the tube just below the upper reservoir. The change in the load due to a change in *g* resulted in a vertical displacement of the yoke and was measured by means of a micrometer screw supported from the lower reservoir. The observations required corrections for temperature and atmospheric buoyancy. The instrument was not checked by testing it at two land stations where the relative value of *g* was accurately known. His results show, however, a remarkable correlation with the depth as obtained by direct soundings, which were made immediately after the bathometer readings. In a series of about 30 observations involving depths up to about 2,500 fathoms, the discrepancy was seldom more than 10 per cent. No corrections were applied for variation in latitude, which ranged from 45° to 49° N.

Hecker's method: The appearance of Helmert's equation for the variation of *g* with latitude, based on land stations, led Hecker[1] to undertake the task of providing data to test its validity for sea stations as well. In 1901 he began an extended series of gravity measurements at sea which eventually included systematic observations in the Atlantic, Pacific and Indian oceans and in the Black Sea.

In using a barometric column for measuring *g*, two procedures are available: (1) the air chamber at the base of the column may be sealed,

[1] For a description of the apparatus employed by Hecker and a summary of his ocean measurements, see Hecker, O., Bestimmung der Schwerkraft auf dem Schwarzen Meere und, an dessen Küste sowie neue Ausgleichung der Schwerkraftmessungen auf dem Atlantischen, Indischen, und Groszen Ozean, *Zentralbur. Internat. Erdmessung*, Veröffentlichungen Berlin, N. F., Nr. 20 (1910).

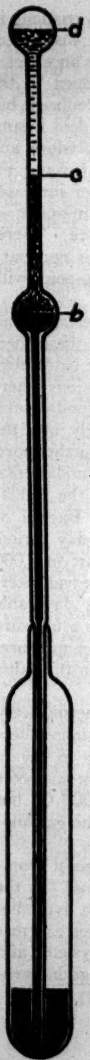

FIG. 1. Siemens' first apparatus.

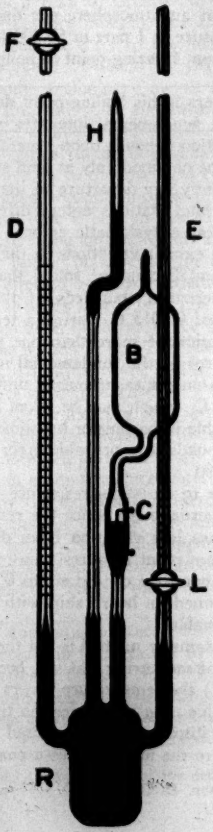

FIG. 2. Duffield's apparatus.

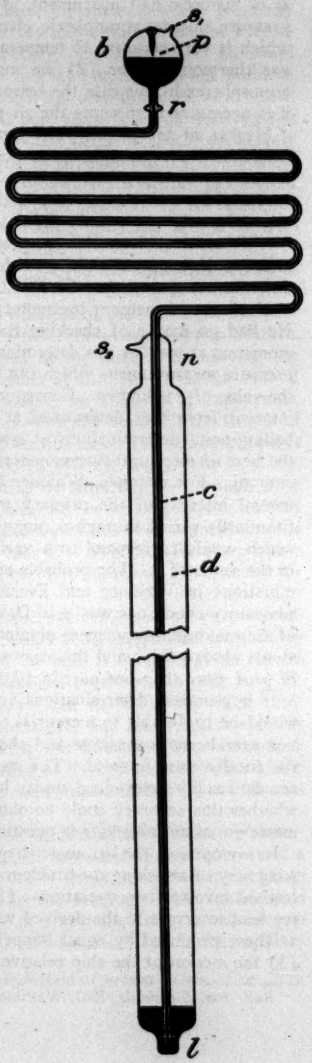

FIG. 3. Briggs' apparatus.

as in Siemens' first instrument, in which event we eliminate variations in pressure due to atmospheric changes, but are left with an instrument which is very sensitive to temperature changes, as it becomes in effect a gas thermometer; or (2) we may leave the air-chamber open to the atmosphere; in this case the temperature effects are greatly reduced, but it is necessary to measure the air-pressure p by some independent means.

Hecker in his pioneer investigations chose the latter procedure and determined the pressure from boiling-point measurements of water referred to vapor-pressure tables. The vapor-pressure of water increases very rapidly with temperature in the neighborhood of the boiling-point,—approximately one twenty-eighth of an atmosphere per degree. Therefore, in order to determine the pressure to 1 part in 50,000 it is necessary to know the temperature interval from freezing-point to boiling-point with an error not greater than 0°.0006 C.

Hecker used mercury thermometers in his boiling-point determinations. He had no means of checking the fundamental intervals of these thermometers at sea, for this determination depends upon accurate barometric pressure measurements which can be obtained only at land stations where the value of g is known. Consequently any departure of the fundamental interval from that determined at land stations enters directly into the boiling-point determinations at sea as a systematic error. Furthermore, the best of mercurial thermometers exhibit variations in the fundamental interval. For example, Waidner and Dickinson[1] found that the fundamental interval of the primary mercurial standards of the Bureau of Standards varied through a range of 0°.015 C. during a ten-day period, which would correspond to a variation of more than one part in 2,000 in the value of g. The probable error of the fundamental interval determinations in Waidner and Dickinson's measurements under favorable laboratory conditions was ±0°.003 C., which may be taken as a measure of the maximum refinement obtainable in barometer-hypsometer measurements aboard ship; and this corresponds to a probable error in the value of g of more than one part in 10,000.

If hypsometer determinations are to be made, resistance thermometry would be preferable to mercurial thermometry, since the resistance thermometer is more sensitive and shows less variation from day to day in the fundamental interval. The steam point of a resistance thermometer can be readily determined under laboratory conditions to 0°.002 C., but whether this accuracy could be obtained on board ship with the galvanometer on an unstable base is questionable.

In my opinion, the barometer-hypsometer method is not the most promising way of attacking the problem of measuring g at sea, because (1) the method involves two operations; (2) the temperature errors in hypsometry lead to errors in the derived value of g which are ten times as great as those produced by equal temperature errors in a closed system; and (3) the motion of the ship relative to the air produces a change in baro-

[1] *Bull. Bur. Standards*, 3663, Washington, D. C. (1907).

metric pressure below deck. If this relative motion is unsteady from any cause, as for example variable winds, errors may result unless the barometer and hypsometer are read simultaneously. Duffield (1921) on board a destroyer observed pressure effects of this kind as large as one millibar.

The use of a sensitive aneroid barometer has been proposed as a substitute for hypsometric measurements, but here again a double operation is involved. If a spring system could be devised which would be sufficiently sensitive and reliable to measure pressure to the required degree of accuracy, it would be better to employ it directly as a force balance to measure the change in g than to equate the observed pressure to the observed length of a mercury column, for the double operation serves only to increase the probable error of the final result.

Duffield's method: Duffield[1] in 1914 employed the apparatus shown in figure 2 in some preliminary measurements of g during a voyage from Australia to England. The apparatus, which is of the sealed gas-chamber type, possesses a unique and valuable temperature-compensation feature. A constant volume of air is maintained in the bulb B by keeping the mercury always up to the electrical contact at C. The air in the bulb B is under reduced pressure in order to reduce the length of the apparatus. The barometer tube is bent so that H is vertically above C, the length of the column HC being approximately 20 centimeters. The mercury level is kept at C by raising or lowering the mercury in the index tube D. This operation is effected by slowly exhausting or admitting air through F as required. The index tube D is of fine bore and the value of g is calculated from readings upon the level of the mercury in this tube when contact is made with the pointer at C. The side tube E is used only for the purpose of making initial adjustments, and to permit the apparatus to be used for various ranges of temperature.

The reservoir of mercury R is introduced for the purpose of temperature compensation and when the dimensions of the apparatus are suitably chosen the increased pressure of air in the bulb B due to a given rise of temperature is automatically counterbalanced by the rise of the level H, occasioned by the expulsion of mercury from the reservoir. The compensation is perfect at only one temperature, but for small departures the error is small. At sea the apparatus was immersed in a water bath which was hung by cords from the ceiling of the refrigerator room of the ship, and readings on the index tube were taken through a window in the side.

Duffield's apparatus as reconstructed in Australia was over compensated, an increase in temperature of 1° C. necessitating the removal of a thread of mercury 60 mm. long. In other words, a temperature change of this amount resulted in a change in the reading of the index scale corresponding to the computed change in g in going from the equator to 54° N. Lat. Trouble was also experienced from a break in the capillary

[1] W. G. Duffield. Apparatus for the determination of gravity at sea. *Proc. Roy. Soc. Lond.* (A), 112, 1916, 505-517.

mercurial column in the barometer tube. Owing to these difficulties actual observations were limited to a series of 25 preliminary measurements in the Indian Ocean from Lat. 0° to 16° N. These results show an average deviation with regard to sign of +0.01 cm./sec.² from values computed from Helmert's equation; in other words, a chance distribution of the observations about the line representing Helmert's computed values is indicated. The average deviation of the observations without regard to sign is 0.14 cm./sec.². The anomalies are, however, not known. If we assume that there are no anomalies, this corresponds to an average error of 1.4×10^{-4}. It is interesting to note in this connection that Schuster computes from the dimensions of Duffield's apparatus a maximum error due to pumping of about 3.6×10^{-4} for vertical motions of the ship of one meter amplitude.

In a recent article Duffield[1] has given a brief description of tests made with apparatus other than his own during his voyage to Australia in 1914. This included instruments of the sealed-cistern barometer type constructed by Prof. Hecker and an aneroid barometer supplied by Sir Horace Darwin. No quantitative results are given.

Schuster[2] has contributed a valuable analysis of the effects of forced vibrations which may be imposed on the mercury in gravity apparatus by the vertical acceleration of the ship. This includes a discussion of (1) a single constricted barometer tube, (2) the oscillations in a complex interconnected system of three tubes as in Duffield's apparatus, and (3) the experimental errors in the latter apparatus as affected by the relative dimensions of the various parts. He emphasizes the importance of the condition that the flow of mercury in the barometer tube and contact tube be such that the difference in level is always that of hydrostatic equilibrium. This condition is fulfilled if the cross-sections of the capillaries are equal and the lengths of the capillaries are inversely as the reduced[3] cross-sections of the tubes at the free surfaces of the mercury.

Briggs' method: Briggs[4] employed apparatus similar to that shown diagramatically in figure 3 for gravity observations during a voyage from Sydney to San Francisco in 1914 and again from New York to San Francisco via Panama in 1915. This apparatus is of the closed-barometer

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[1] The investigation of gravity at sea. *Nature*, 106, 1921, 732-734.

[2] Arthur Schuster. On the determination of gravity at sea (Note on Dr. Duffield's paper). *Proc. Roy. Soc. Lond.* (A) 112, 1916, 517-528.

[3] The reduced cross-section a represents the actual cross-section a corrected for the effect of the pressure and volume of the air above the surface of the mercury. Let

V=the original volume of air.

P=the original air pressure.

h=the height of the mercury column equivalent to P.

$$\text{Then } \frac{1}{a_1} = \frac{1}{a} + \frac{h}{V}$$

[4] L. J. Briggs. A new method of measuring the acceleration of gravity at sea. *Proc. Nat. Acad. Sci.* 2, 1916, 399-407.

type. The mercurial column is contained in the capillary *c* (bore 0.6–0.7 mm.), the lower end of which opens beneath mercury in the bottom of the gas chamber *d*. This capillary is sealed to the wall of the gas-chamber where it passes through the upper end. The upper part of the capillary is bent into a flexible zigzag and ends in the spherical bulb *b* (diameter 2 cm.). The bulb contains a fixed iron point *p* sealed to the inside of the bulb by means of an inserted platinum wire and extending vertically downward, so that the point is approximately at the center of the bulb. The length of the mercurial column is about 74 cm.

The flexible capillary permits a slight vertical movement of the observing bulb with respect to the gas chamber. This movement is determined by a micrometer screw of 1 mm. pitch which controls the motion of a carriage in which the observing bulb is rigidly mounted. The carriage slides on parallel rods mounted on a base which is rigidly cemented to the neck *n* of the gas chamber, so that the position of the bulb relative to the gas chamber is definitely determined by the screw.

The apparatus is protected by a close-fitting metallic jacket, and is kept at a constant temperature in a bath of melting ice. It is necessary to determine only the position of the upper end of the barometric column. The design of the instrument is such that in setting the index in contact with the mercury surface the enclosed gas is automatically reduced to a constant volume; and since the temperature is constant, all measurements are made at constant pressure. The relative value of g at two stations is thus inversely proportional to the observed length of the column at these stations.

The contact of the index point with the mercury surface can be determined either electrically or by direct observation. Both methods were used. In the latter case the fixed point was observed through a glass tube introduced through the ice, the tube containing a low-power lens. The point was illuminated through a similar tube on the opposite side. If the sea was so rough as to cause pumping of the column, the point was so adjusted that it was in contact half the time as nearly as possible. Since the motion of the ship is not strictly periodic, there is considerable uncertainty connected with such settings.

This instrument possesses the following features which experience has shown are desirable in gravity apparatus: (1) The glass part of the apparatus is hermetically sealed and can be made really gas-tight. There are no stop-cocks, ground joints, or mercury seals. (2) It is necessary to make settings only at the upper end of the barometric column. (3) This permits the complete immersion of the apparatus in an ice-bath, which is the most dependable source of constant temperature for use on shipboard. (4) The apparatus is portable, since at room temperatures the pressure is sufficient to fill the observing bulb with mercury.

Gravity determinations which were made in 1914 on board ship in Wellington harbor and Sydney harbor using observations in San Francisco harbor as a base station, and in 1915 in San Francisco harbor using New

York harbor as a base, show an average departure from pendulum observations of about one part in 50,000. These observations were made under favorable conditions and serve to show the degree of accuracy with which the instruments held their adjustments during the long voyages rather than to provide any indications of the accuracy of the sea observations. In fact, the publication of the sea measurements, which show some rather large anomalies, has been withheld in the hope that apparatus similar to that described in the last section of this paper might be available to determine the errors of the instruments under oscillations approximating sea conditions.

CORRECTIONS FOR THE COURSE AND SPEED OF THE SHIP

Eötvös[1] has shown the necessity of applying a correction for the easterly or westerly motion of the ship, due to the fact that the ship's motion modifies the angular velocity of the apparatus about the earth's axis. The centrifugal force acting on the mercurial column when on board a ship moving east or west is therefore not the same as when the ship is at rest or moving north or south. The correction may be as great as 1 part in 10,000, but can be accurately computed if the course, speed, and approximate latitude of the ship are known. Duffield (1921) observed a change equivalent to 0.1 millibar when the course was altered from east to west when steaming at 22 knots, which corresponds to an apparent change of 1 part in 10,000 in g. He does not state where his experiments were made. For Lat. 55°, which represents approximately the mean latitude of the North Sea, the computed change is about 0.19 cm./sec.^{2}, or 2 parts in 10,000 in the value of g.

LABORATORY TESTING OF GRAVITY APPARATUS

There seems to be no practical way of determining the accuracy of gravity measurements directly from sea observations. With a ship at one's command, repeated traverses could be made of the same sea station under varying sea and weather conditions; but while this would provide a measure of the accidental errors, it would tell us nothing regarding systematic errors, for the exact value of g at the station would not be known. The value of g in a long, narrow bay could be closely approximated from pendulum observations on both shores. To satisfy other requirements the bay would have to be sufficiently open and windswept to represent the conditions prevailing in a moderate sea. Such a test seems precluded without the enlistment of government aid.

We can, however, simulate sea conditions at a land station where g is accurately known and I wish to emphasize the importance of such tests for all gravity apparatus. Suppose we construct a platform capable of independent reciprocating horizontal translations in two directions at right angles, corresponding in period to the roll and pitch of the ship. Let us mount upon this platform a second one arranged for vertical oscillations,

[1] See Helmert, *loc. cit.*

large enough to carry the apparatus and the observer. All the reciprocating motions are to be capable of a continuous change in amplitude if desired in order to secure the conditions which arise when the period of the ship differs from that impressed by the waves. We have omitted the motions corresponding to the angular motions of the deck, for structural reasons; for these angular motions would only increase the rotation of the apparatus in its gimbals, and there will always be sufficient movement of the gimbals arising from the horizontal accelerations to simulate disturbances due to friction. With this apparatus it will be possible to investigate the effects of horizontal and vertical accelerations, singly and combined, under conditions where g is accurately known and thus obtain a measure of the accidental and systematic errors of the method. Independent rolling and pitching oscillations are probably not required for gravity apparatus, but the equipment proposed has other useful applications, as for example in testing gyroscopic compasses and gyroscopic stabilizers, in which both rolling and pitching accelerations should be included. Plans for such an equipment are now being prepared, and it is hoped that arrangements can soon be made for its construction and installation at the Bureau of Standards, where it will be available for testing all gravity apparatus.

Bureau of Standards,
Washington, D. C.

ISOSTASY

By John F. Hayford

I assume that I am expected to bring forward whatever ideas will, in my opinion, help most at the present time in the development of a study of isostasy by stimulating further thinking along that line. From among the many topics which might be treated, I am selecting three:

(1) I shall make some remarks on the recent paper entitled "The Chemistry of the Earth's Crust," by H. S. Washington.

(2) I shall try to emphasize the desirability of an intensive study of two small areas by observations of gravity and deflections of the vertical.

(3) I shall offer some considerations that lead me to believe that the undertow involved in isostatic readjustment is above, rather than below, the depth of compensation.

CHEMISTRY OF THE EARTH'S CRUST

Dr. Washington, in his paper entitled "The Chemistry of the Earth's Crust," [1] has set forth a piece of research of much importance to those who are studying isostasy. He has set forth the evidence derived from studies of the densities of igneous rocks. These densities are determined from chemical analyses. In the latter part of the paper he sets forth the correlation which is observable between the density of igneous rocks, on

[1] *Journal of the Franklin Institute*, 190, December, 1920, 757-815.

the one hand, and, on the other hand, the elevation of that part of the earth's surface under which the rocks lie.

The evidence given in the paper seems to be conclusive in its general features on three points, namely:

(1) That igneous rocks under the oceans are denser than those under the continents;

(2) That igneous rocks under the various continents are less dense the greater the mean elevation of the continent; and

(3) That igneous rocks under different parts of any one continent are less dense the higher is that part of the continent.

All three of these conclusions are in accord with the theory of isostasy and, in Dr. Washington's words, constitute "almost a conclusive proof of the general validity of the theory of isostasy."

Dr. Washington seems to interpret the relations pointed out as being due to original, or early, segregation of the material. I do not question this interpretation so far as (1) is concerned. The relative positions of the oceans and continents are permanent or semi-permanent. It may well be that the oceans are now in their present position because material of such a nature as to form dense rocks was placed early in the positions now occupied by the oceans.

The same interpretation may, however, be questioned, in part, in so far as (2) and (3) are concerned. Are the differences in densities between the different continents referred to in (2) and between different parts of any one continent referred to in (3) due to original or early segregation? Or are these differences due, in part at least, to some response of the material to a change of pressure in such a manner as to bring about a change of density? The continents probably have not had the same relative elevations throughout geologic time as they now have. Certainly, the different parts of any one continent have not, in general, the relative elevations now that they had at various times during the geologic history of the continents. For example, the Appalachian region, referred to by Dr. Washington, in the eastern part of the United States, is now much lower than Utah, Colorado, and Nevada, and the density of the material, as measured by him, is considerably greater under the Appalachian region than under the states named. There was, however, a time during the geologic history of the North American continent when the Utah-Colorado-Nevada region was much lower than the Appalachian region was at that time. Have the relative densities in the two regions changed between that period and the present?

This comment on Dr. Washington's paper is not intended at all as a criticism; it is intended to supplement the paper and to stimulate further thought based on it. All who are interested in isostasy should be very keen to follow the further develpoments along the line indicated by Dr. Washington's paper. I understand that he, himself, will develop the evidence much more fully. The more complete evidence should be examined very carefully, with a view to determining the bearing of this evidence on

any theories which may be held in regard to isostasy and the isostatic readjustment.

PROPOSED INTENSIVE STUDY OF SMALL AREAS

We shall take a short time to consider the possible benefits which would follow from an intensive study of two small areas, let us say about 100 miles square, one on rather flat country, such as Louisiana, and another in hilly or mountainous country. The intensive study would be based on closely spaced stations of two kinds, stations at which deflections of the vertical are determined and gravity stations. Let us suppose that observations of these two kinds were made in each of two such areas and then that, by the proper office methods and by the combination of the two kinds of observations, the distribution of the densities beneath each of the two areas was determined with as great accuracy as is possible. What benefits would follow from two such studies? Briefly, I believe that the benefits would be as indicated in the four paragraphs which follow:

1. The studies would bring out the actual advantages and limitations of this line of attack on the problem of determining the distribution of densities beneath the earth's surface. The relative strength or weakness of this line of attack, as compared with the usual line of attack, is not now well known. The usual line of attack is to use deflections of the vertical stations or gravity stations which are widely distributed somewhat uniformly over a very large area.

2. The studies would probably furnish a considerable amount of evidence on the point which has been ably brought out by Dr. William Bowie that the small anomalies in gravity and in deflections of the vertical which remain after correcting for topography and isostatic compensation are closely related in many regions, if not as a rule, to the surface geology of those regions. It is important that the extent to which this is true should be determined. There seems to be no doubt that it is true, in a general way, for large areas. The question is, to what extent is it true for small areas?

3. The studies would give valuable indirect evidence as to the extent to which the present conclusions from the evidence now available are vitiated by local effects which are at present unavoidably assumed, for want of more exact information, to extend half way to the next station, so to speak.

4. The two intensive studies would help to determine whether oil and salt may be located by geodetic measurements. This suggestion has been made at various times by Dr. David White, Chief Geologist of the U. S. Geological Survey. It is important to know whether gravity stations and deflections of the vertical may be used as a divining rod for that purpose with sufficient accuracy. If the accuracy of such a divining rod is so low that its indications are likely to be misleading, then it is not feasible to try to use the observations in this way. On the other hand, if two such intensive studies as are indicated here show that sufficient accuracy is

possible, then certainly the method should be used, in combination possibly with other methods. The two intensive studies might possibly be made in regions in which salt or oil are believed to exist, or are known to exist, in large quantities.

The suggestion that one study should be in rather flat country and the other in rather hilly country is based on two considerations. In very flat country the conclusions reached will be vitiated to a much smaller extent by an error in the assumed surface density of the material than in rough country. On the other hand, in hilly or mountainous country the depth at which any material of abnormal density lies may be determined with greater accuracy than in flat country.

DEPTH OF UNDERTOW

Let us now turn to the question, is the undertow involved in isostatic readjustment above or below the depth of compensation?

Assume that, at some time in the remote geologic past, the North American continent and its various major parts have been almost completely compensated in the isostatic sense. Assume that, in later geologic past time and up to the present time, there have been large amounts of erosion from large portions of the continent and corresponding large amounts of deposition in other parts. Assume that in that period and up to the present, readjustment toward isostatic conditions has been in progress by horizontal transfer of material from beneath the regions of deposition towards the regions of erosion.

It is reasonably certain that, on the whole, these three assumptions are true. The question on which it is desired to concentrate attention is, has the horizontal transfer taken place below or above the depth of compensation?

It is important to secure the correct conclusion on this point, whatever it is, because the apparent correlation of geodetic and geologic evidence, or the apparent contradictions between two lines of evidence, probably depend somewhat intimately upon the conclusion reached. I believe that the horizontal transfer has taken place above the depth of compensation, say within 100 kilometers of the surface, rather than below that depth. I propose to state, very briefly, some of the lines of thought that have led me to that conclusion.

If it is assumed that, under an area of deposition, the material down to the depth of compensation all sinks under the added load and that the horizontal transfer of material occurs below that depth, the case is similar to that of an ice floe. Under each elevation on the upper surface, in this case, there must develop a much larger bump on the lower surface of the floating mass. The conception is that of a crust floating on a relatively plastic stratum. The level of compensation, in this case, is at the lower side of the floating crust. As in this case there must be extensions of the crust downward, below the mountains, the depth of compensation will be variable, being great under high areas and small under low areas. The

geodetic evidence, as far as I have been able to examine it, does not seem to be conclusive that there is any such relation between the depth of compensation and the elevation of the surface. This leads me to be skeptical of such theory, which involves a horizontal flow limited mainly to those portions of the earth that are below the depth of compensation.

In general, I am skeptical of any explanations of isostatic readjustment, or of other phenomena in the earth, which involve a relatively plastic stratum in contrast to more rigid material above. In each of the cases in which I have been able to follow the mechanics of the problem to my own satisfaction, I have not been convinced that the resort to the device of introducing a plastic layer into the concept is necessary. So, in the problem now under consideration, which is that of the isostatic readjustment, I do not find it necessary to assume any stratum to be more plastic than the one above, in order to harmonize the observed facts of various kinds.

There is abundant geological evidence of horizontal stresses and strains in the earth's crust. This evidence seems to me to be conclusive. The geologic evidence seems to me to indicate a horizontal transfer of material during isostatic readjustment relatively near the surface, rather than at great depths. If the horizontal transfer involved in the undertow were in a plastic stratum more than 100 kilometers below the surface, certainly the horizontal stresses set up in the surface material would be much less than if the same transfer occurred in less plastic material nearer the surface.

In attempting to determine the mechanics of the isostatic readjustment which apparently takes place when great loads are removed from a region of erosion and equally great loads are added to other regions as deposited material, it is extremely important to keep in mind that material deforms under relatively small differences in the two principal stresses. A very large increase in both the principal stresses is necessary in order to produce deformation if the increases are equal. In an elementary cube of the material, let p_2 be the pressure on the upper and lower faces of the cube. Let p_1 be the pressure on the side faces of the cube. If $p_2=p_1$, the material is under isostatic conditions. Under these conditions both p_2 and p_1 may be increased very largely before appreciable compression is produced in such material as constitutes the earth's crust. On the other hand, if p_2 is increased without changing p_1, then deformation of the cube will be produced for a relatively small difference p_2-p_1, corresponding to an added load of a few thousand feet only of material. The cube will be deformed in the sense in which the vertical dimension is decreased and the horizontal dimensions increased. Consider the movements which will take place if the many elementary cubes under a large loaded area are so distorted. Evidently the motions of the material under the margins of such a load will have a horizontal component. This line of thought, followed through to its logical conclusions, and made more definite by careful analysis, will indicate that the horizontal transfer of material occurs largely at moderate depths, certainly at less than 100 kilometers as a rule.

I am perfectly aware that the presentation of the considerations set forth in the last few paragraphs has been too brief for clearness or conclusiveness. The paragraphs have been written as suggestions rather than as demonstrations. They indicate lines of thought which should be followed up carefully if one wishes to reach true conclusions.

I desire to reiterate my opinion,[1] based on such thought as I have been able to give to the subject, that the undertow involved in isostatic readjustment is above the depth of compensation.

THE EARTH-TIDE EXPERIMENT

By Henry G. Gale

I understood Professor Hayford to say that he did not believe that a fluid layer exists beneath the solid crust of the earth. The same conclusion may be drawn from the earth tide experiment which was conducted on the grounds of Yerkes Observatory by Professor Michelson and myself. It seems pretty certain that the earth tides are the same as they would be if the earth were a highly elastic homogeneous solid, both with respect to the phase and amplitude of the earth tides.

The experiment was entirely successful from a physicist's standpoint. The interferometers gave very little trouble. One of them did not require readjustment during the entire year. Two were readjusted to change the width of the fringes, and on one interferometer it was necessary to resilver one of the mirrors. As a source of light we used commercial alternating Cooper-Hewitt lamps, and they proved to be entirely satisfactory. The only serious interruptions were caused by breaks in the electric service due to storms, and occasional short shut-downs by the electric light company which supplied the current. The experiment at Yerkes Observatory was continued for just one year. This is probably long enough to give values of the semi-diurnal and diurnal tides, accurate to a few tenths of one percent. For tides of longer period the experiment should be continued for, say, five years, although possibly three years would be long enough.

The experiment is now being repeated on the grounds of the California Institute of Technology at Pasadena, California. One additional station should be installed, preferably on an island in the Pacific, far from the continental borders. If the three stations should give results in agreement on both the phase and amplitude of the earth tides, I should feel that the problem had been solved. If the three were not in agreement, at least one-half dozen additional stations would be desirable. They should be well scattered in latitude, and with reference to tidal coasts.

The cost of installing a station is not excessive, and one man can easily keep a station in operation and reduce the observations. It would probably be worth while to look for a correlation between the slight changes of

[1] As early as 1911 I had reached the above conclusion, as indicated by one of the diagrams in an article published in *Science*, 23, No. 841, 199-208, Feb. 10, 1911, entitled "The relations of isostasy to geodesy, geophysics and geology."

level often shown by the apparatus and the approach or passage of the intense barometric lows and highs of large area. It would certainly be worth while to install a specially designed apparatus, similar to that used for the earth tides, to detect and measure the rate of tilting in the surface layers of the earth's strata at especially favorable places. A relation between such rates of tilting and earthquakes might be detected.

THE EÖTVÖS BALANCE

By W. D. Lambert

There is one instrument of use in the study of terrestrial gravity which has not yet been employed in the western hemisphere, though it has to a limited extent in Europe. This instrument is the Eötvös balance. It should be tried, I believe, in making the proposed minute investigation of gravity in a level region of limited extent.[1] To judge by accounts of work done with the balance in Europe it would certainly supplement the pendulum advantageously in the proposed gravity survey and might largely supersede it. This does not by any means signify, however, that the balance is always and everywhere a substitute for the pendulum. This is not the occasion for an exposition of the principles of the instrument, but it may be said that the Eötvös balance connects the results at adjacent points in a limited region with one another in a way that the pendulum cannot well do.

In Europe they evidently believe in its possibilities as an indicator of the existence and location of concealed irregularities and discontinuities in density. Some of these discontinuities may mean strata of geological or commercial interest. At least three attempts have been or are being made to locate such strata by the use of the balance (1) by Dr. Schumann to locate lignite deposits in Austria,[2] (2) by Professor Schweydar for geological purposes in the region about Hamburg, Germany,[3] and (3) an attempt to locate salt deposits in Poland.[4] It will be of interest to know how successful these attempts prove to be.

The Eötvös balance determines certain second derivatives of the gravity potential function. It does not, however, determine the second derivative in the vertical direction. This quantity has to be determined theoretically. This is the most serious deficiency of the instrument and explains why it is of comparatively little use in rough country. Attempts are being made by Berroth, Helmert's assistant, in his last researches on gravity, to devise a means of determining experimentally the second derivative in the vertical direction.[5]

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[1] See p. 13 of this bulletin.

[2] *Akademie der Wissenschaften in Wien:* math. phys. Klasse. Sitzung vom 8 Janner, 1920. Reported in the *Akademische Anzeiger*, Nr. 1.

[3] "Rapport sur les Travaux du Bureau Central de l'Association Geodésique Internationale en 1920," p. 4.

[4] I have seen nothing in print about this third attempt. I have word of it personally from the man proposing to make the observations. [Added in proof: This attempt has not yet been made on account of the lack of instrument.]

[5] Same reference as in second preceding footnote.

THE PROBLEM OF THE EARTH TIDES

By W. D. Lambert

There are two methods of attacking the problem of the elastic properties of the earth, (1) the study of the seismological evidence, on which I shall not touch at all, and (2) the study of earth tides. Even after the best available observational evidence from the earth tides has been obtained, a good deal of hypothesis and interpretation is required before we can say: "The elastic constants of the earth are thus and so." It is not, however, of this that I wish to speak, but rather of the problem of obtaining the true values of the earth tides. This subject is connected with several other geophysical questions and this paper, therefore, falls naturally into three divisions: (1) Earth tides and the long-period oceanic tides; (2) earth tides and the short-period oceanic tides; (3) earth tides and the variation of latitude.

EARTH TIDES AND THE LONG-PERIOD OCEANIC TIDES

The first quantitative evaluation of the earth tides and hence of the elastic properties of the earth came from a discussion of the so-called long-period oceanic tides. The suggestion which initiated the work appears to have come from Lord Kelvin and the method and results are given in Thomson and Tait's "Natural Philosophy,"[1] but the actual discussion of the observations was made by Darwin.[2]

The discussion is based on the assumption that the long-period oceanic tides for an ocean on a perfectly rigid globe may be calculated on the equilibrium theory. This means that it is assumed that the disturbance of equilibrium caused by the tide-producing forces of long period can travel so rapidly through the water and the forces themselves change so slowly that the ocean has time to adjust itself to the forces and that at any given instant the surface of the ocean forms an equipotential surface for the instantaneous field of force.[3] The observed oceanic tide would be the difference between the oceanic tide for a rigid body and the earth tide, and when the first two are known the earth tide may be inferred. From 33 years of observation on both the monthly and the fortnightly tides at 14 different ports Darwin deduced that the observed tides were about two-thirds as large as they would be on a perfectly rigid globe. The earth tide corresponding to the other third implies an effective rigidity of the earth about equal to that of steel.[4] Later methods of attacking the

[1] Second Edition (1883), § 848.

[2] G. H. Darwin: "Scientific Papers," I, 340. This contains a reproduction of the passage in Thomson and Tait with some changes of notation and unimportant omissions.

[3] This assumption is very evidently incorrect as regards the diurnal and semi-diurnal tides. It was supposed to be at least approximately true for the monthly and fortnightly tides.

[4] The tides at Indian ports, which are more consistent with one another than the others, gave a much higher rigidity.

problem have given results somewhat similar, though in general tending towards somewhat higher values of the rigidity.

Only a few years passed before this result was questioned because of its dependence on the equilibrium theory, and Darwin himself, apparently, was the first to question it.[1] In treating the problem of tides on a globe covered with water—a problem first formulated by Laplace—he discovered solutions which for depths anything like these of our actual oceans gave tidal oscillations of perhaps half the amount deduced from the equilibrium theory. He concludes his article by saying: "Thus it does not seem likely that it will ever be possible to evaluate the effective rigidity of the earth by means of tidal observation." The mathematical treatment of the problem has since been developed by Hough[2] and Goldsborough.[3] The latter has extended the treatment to include basins bounded by two parallels of latitude, a polar basin or one covering the entire globe being special cases. The general result is that for oceanic depths such as we know the monthly and fortnightly tides in such basins differ considerably from what the equilibrium theory gives, being in general considerably smaller.

These results are decidedly puzzling when compared with observation, for the observed tides would then be larger than the computed tides of Darwin, Hough and Goldsborough instead of being smaller by an amount representing the yielding of the earth to the tide-producing forces. Darwin's solution was re-examined critically by the late Lord Rayleigh.[4] He reaches the conclusion that the solution is a very special one that applies, of course, to the ideal water-covered globe postulated, but which has little relation to our actual oceans, interrupted as they are by continental barriers. He says: "If this conclusion be admitted, the theoretical fortnightly tide will not differ materially from its equilibrium value, and Darwin's former calculation as to the earth's rigidity will regain its significance." After a word of caution about possible exceptional conditions he concludes: "In any case I think that observations and reductions of the fortnightly tide should be pursued. Observation is competent to determine not merely the general magnitude of the tide but the law as dependent upon latitude and longitude. Should the observed law conform to that of the equilibrium theory, it would go a long way to verify *à posteriori* the applicability of this theory to the circumstances of the case."

Rayleigh's belief in the legitimacy of calculating the fortnightly tide—and *à fortiori* the monthly tide—from the equilibrium is supported by the opinion of Love[5] and there is still another reason for accepting this idea,

[1] *Proceedings of the Royal Society of London*, 40 (1886), 337, or "Scientific Papers," I, 366.

[2] *Philosophical Transactions of the Royal Society of London*, 189, 1897, 201, and 191, 1898, 139. Some account of Hough's work is given in Darwin's article on "Tides" in the eleventh edition of the Encyclopædia Britannica, parts of which are given also in Darwin's "Scientific Papers," I, 347.

[3] *Proceedings of the London Mathematical Society*, Vol. for 1914-15, 31 and 207.

[4] *London, Edinburgh and Dublin Philosophical Magazine*, 5, 1903, 136.

[5] "Some Problems in Geodynamics" (Cambridge, England, 1911), 51.

namely, the effect of friction. Hough[1] evaluates the modulus of decay under friction for various types of oscillation. The modulus of decay is a *time* of the same order of magnitude as the period of an oscillation that is slow enough to conform approximately to the equilibrium law. For oscillations most nearly corresponding to the long-period tides and with the laboratory value of the coefficient of viscosity of water he found moduli of the order of magnitude of ten years. But this use of the laboratory value of the viscosity seems to be fallacious. The laboratory value applies to the so-called laminar motion, while the motion in the actual ocean is turbulent. When we call the motion turbulent, we say in effect that we do not understand it very well; but it is known that if we attempt to represent turbulent fluid motion by equations of the same form as are used for laminar motion, then the coefficient of viscosity in the latter, which is the laboratory coefficient, must be replaced by a coefficient of virtual viscosity many times greater. Ekman found for water that the coefficient of virtual viscosity was 15,500 times greater; for air, Taylor found the ratio of the virtual viscosity to the laboratory value to be between 6,000 and 50,000.[2] It is not to be supposed that any one ratio of virtual coefficient to laboratory coefficient would apply under all conditions, but if any such virtual coefficient as is here suggested were used instead of the laboratory value, Hough's modulus of decay would be greatly reduced and the equilibrium theory would apply to tides of comparatively short period, even apart from the effect of continental barriers.

It seems to me, then, that it is pretty safe to assume that the monthly and fortnightly tides conform quite closely to the equilibrium theory and that it would be well worth while to resume the study of these tides for the light that they may throw on the rigidity of the earth. The only discussion of the observations since Darwin's original one is due to Schweydar,[3] in which are discussed observations at 43 ports covering 194 years. There must be available among all the harmonic analyses of tides that have been made since that time a great deal of material still unutilized.

In regard to the utilization of this material and the procurement of new material there are three suggestions I should like to make.

First: In securing the observations care should be taken that the tide gauge has a firm foundation. Tide gauges are frequently located on docks supported by piles, or on made ground. If the cost could be afforded, it would be desirable to place them, if necessary, at some little distance from the water in order to get a good foundation, and to connect the gauge with the water by a large pipe. Incidentally, if a firm foundation

[1] *Proceedings of the London Mathematical Society*, 28, 1896, 264.

[2] These values are taken from a paper by McEwen: Ocean Temperatures, their relation to Solar Radiation and Oceanic Circulation; miscellaneous Studies of Agriculture, Biology, Semicentennial Publications of the University of California. Other examples of very wide differences between laboratory and field values are also given by McEwen. The work of Ekman and Taylor is found respectively in the *Arkiv for Matematik, Astronomi och Fysik* 2, 1905, 1. and in the *Philosophical Transactions of the Royal Society of London*, 215, Ser. A, 1915, 1.

[3] *Beiträge zur Geophysik*, 9, 1908, 64.

were assured, the tide records would be valuable in studying the secular rising and sinking of the coast. Too many long series of tidal observations that would have been valuable for this purpose in the past have been rendered useless for lack of connection with well-established permanent marks. Schweydar deduced from his discussion of the tides an effective rigidity of the earth rather less than that of steel and suggests that the discrepancy between this result and the higher rigidity obtained from the variation of latitude may be due to a plastic stratum beneath the crust. He seems to have abandoned later his belief in this plastic stratum. Perhaps such a stratum exists, after all, but it may consist simply of the alluvial ground, mud or made land on which some of the tide gauges are situated.

Second: In reducing a year's tidal observations at a place to obtain the long-period tides it is found that the tides are much entangled with one another. To separate them Darwin gives a rather tedious process of successive approximations. It is possible to dispense with these repetitions at the price of a rather heavy piece of preliminary computation that is done once for all series of a given length, such as a year. If many long-period tides are to be reduced, it would be well worth while to do this preliminary work. Furthermore, Harris has suggested that if four consecutive years be taken together, the several long-period tides will separate satisfactorily from one another and the computation will be simpler.

Third: In the final discussion we seek to obtain the earth tides from the difference between the theoretical tides on a rigid globe and the observed tides. Now, even accepting the comparatively simple equilibrium theory, which we are at present supposing to be adequate, the tides on a rigid globe have not been evaluated as accurately as could be wished. The difficulties are the attraction of the water on itself and the presence of the continents. Either difficulty by itself is readily overcome. If there were no continents and the globe were covered with water, a simple factor derived from the fundamental principles of spherical harmonics would take care of the self-attraction of the water. On the other hand, if we disregard the self-attraction of the water, the influence of the continents can be allowed for as Darwin and Turner[1] have done in following Sir William Thomson's (Lord Kelvin's) suggestion.

It is the combination of the two circumstances, neither of which is troublesome by itself, that makes trouble, for the resultant of the two cannot be had by simply superposing two corrections. The total effect cannot be very large, but the modulus of rigidity is rather sensitive to changes in the ratio of the observed tide to the theoretical tide, especially if this ratio be near to unity, so that it seems perhaps quite possible that the modulus of rigidity may be changed as much as thirty or forty percent by the application of the corrections above mentioned.

[1] *Proceedings of the Royal Society of London*, 40, 1886, 303, or Darwin's "Scientific Papers," I, 328.

The evaluation of this correction of the combined effect of the continents and the self-attraction of the water seems to me a problem worthy of study. No advance seems to have been made since Poincaré's paper,[1] in which he works out a solution very elegant in conception, but one which, as Poincaré himself says, would lead to calculations far too complicated to be practicable even if the shore line of the continents were arbitrarily simplified into a rude approximation to its actual form. I believe, however, that it may be possible to solve the problem numerically by a laborious process of successive approximations, involving the preparation of maps showing the equilibrium tide corrected for the continents alone, and the reading of these maps for many points on the earth, much as contour maps are read to obtain the deflections of the vertical. The labor would be more than an individual investigator would care to undertake, but might well be within the means of an institution. Perhaps the theory of integral equations in its recent developments might afford means of lightening the labor.

EARTH TIDES AND THE SHORT-PERIOD OCEANIC TIDES

In discussing observations of earth tides as a means for obtaining the elastic constants of the earth—observations made either with horizontal pendulums or with Michelson's tube and interferometer—it is important to have a knowledge of the oceanic tides. This statement applies to tides of all periods, but these remarks apply more particularly to tides of short period, i. e., to the diurnal and semidiurnal tides. Tides of longer period have just been discussed.

The shifting mass of tidal water exerts a direct gravitational effect on the horizontal pendulum or on the liquid in the tube, and the direct effect is reinforced by the tilting of the earth's crust under the shifting load of tidal water. The periods of these effects are precisely the periods of the tide-producing forces; hence it is impossible to make an adequate estimate of the yielding to the tidal forces of the solid earth as a whole until the direct and indirect effects of the oceanic tides have been allowed for.

Probably the most satisfactory determination of the earth tides is that of Michelson and Gale,[2] and it is noteworthy that their observations were made at Williams Bay, Wisconsin, which is some 800 miles distance from the ocean. The yielding of the earth deduced from the north-and-south displacements is nearly the same as that deduced from the east-and-west ones and the yielding deduced from the diurnal declinational tide, O_1, is nearly the same as deduced from the principal semidiurnal tide, M_2.

This satisfactory state of affairs no longer obtains with some of the observations taken elsewhere with the horizontal pendulum. Hecker's

[1] *Journal de Mathématiques Pures et Appliquées*, 2, 1896, 57.

[2] *Journal of Geology*, 22, 1914, or the identical article in the *Astrophysical Journal* for March, 1914. An important correction is given in *Science*, 50, 1919, 327. Definitive results will be found in the *Astrophysical Journal* for December, 1919.

results at Potsdam,[1] which seemed to show a greater yielding of the earth in the meridian than in the prime vertical, have been a standing puzzle; attempts to explain this peculiarity as due to the rotation of the earth have been unsatisfactory.[2]

More recent observations by Schweydar with horizontal pendulums at Freiberg in Saxony[3] show anomalies also. Perhaps these anomalies are due to the inferior accuracy of the horizontal pendulum as compared with Michelson and Gale's apparatus, but it seems to me probable that the uneliminated effects of the oceanic tides may also play a part.[4] This effect has never been calculated, so far as I know, and has been assumed to be so small as to be practically negligible, but, as we shall shortly see, there are reasons for questioning this assumption.

The only serious attempt to allow for the oceanic tides, so far as I know, is due to Prof. Shida[5] of Kyoto University, Japan. He observed with horizontal pendulums for a year at Kamigamo Geophysical Observatory, near Kyoto. In Japan it is, of course, impossible to get far from tide water, and though the tides in the surrounding waters are not particularly large, but rather the contrary, still considerable effect is to be looked for. Harris's cotidal maps[6] were used for the M_2 component. They are based on actual observation for the coast, but necessarily on theory and inference for the open sea. For the O_1 component special maps were drawn from the somewhat meager data available. From the maps the gravitational effects of the tidal water was read off just as the topographic deflection of the vertical may be read. The effect due to the yielding of the earth's crust was also computed on the most plausible assumptions practicable. The actual computation is not unlike that of the deflection of the vertical; the result comes out that the deflection due to the tilting under the tidal load is about twice the direct gravitational pull of the load itself, and the two corrections combined were of the same order of magnitude as the earth tides proper. For example, the observed O_1 tide on the pendulum swinging northwest–southeast came out

$$+0''.00525 \cos t + 0''.00059 \sin t$$

while the total deflection in the same direction due to the ocean tides was computed as

$$+0''.00410 \cos t - 0''.00250 \sin t$$

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[1] "Beobachtungen an Horizontalpendeln über die Deformation des Erdkörpers unter dem Einflusz von Sonne und Mond," Veröffentlichungen des Konigl. Preusszischen Geodätischen Institutes n. f. no. 32, Berlin, 1907, and Heft 2, n. f. no. 49, Berlin, 1911.

[2] See Love: "Problems in Geodynamics" (Cambridge, England, 1911), 75.

[3] "Bericht über die Tätigkeit des Zentralbureaus der Internationalen Erdmessung im Jahre 1920," 6.

[4] Orloff's observations at Dorpat (reported in *Astronomische Nachrichten*, 186, 1910, 81) show a peculiarity similar to Hecker's but to a less degree. Dorpat is farther removed than Potsdam from the influence of the large tides in the Atlantic.

[5] "Memoirs of the College of Science and Engineering," Kyoto Imperial University, IV, no. 1 (Nov., 1912).

[6] Manual of Tides. Part IV B (Report of the U. S. Coast and Geodetic Survey for 1904, appendix 5).

where t is the hour angle of the fictitious O_1 tide-producing body of the harmonic analysis. Evidently any inference drawn from the uncorrected earth tides would be quite wide of the mark. The computation was extended to a distance of 40° of great circle (nearly 2,800 statute miles) from the station. This was considered sufficient in view of the meagerness of the tidal data and the approximate nature of the work, but it should be remarked that the zone between 30° and 40° gave in some cases a result equal to more than one-tenth of the whole correction, suggesting the desirability of extending the calculation to even greater distances.

When we consider the great areas of ocean that lie within 40° of any one of the European horizontal-pendulum stations, it seems rash to assume without careful calculation that we may neglect the effects of the oceanic tides on the observed earth tides, and it may even appear desirable to see whether Michelson and Gale's result may not be susceptible of improvement by applying the correction for oceanic tides.

The primary difficulty with calculations of this sort is our lack of knowledge of tides in the open sea. We have Harris's cotidal lines, and it may be said that when these were used for reducing the Kyoto observations the results obtained seem quite satisfactory, thus verifying to a certain extent the theories on which the lines were based. But at best this is theory rather than observation, and Harris himself was as keenly aware as anyone else of the incompleteness of his work and the necessity of verifying it by observation.

The direct observation of tides at sea is a problem beset with difficulties. To observe tides by means of soundings repeated every hour or so at the same point seems impracticable on account of the great depth to be sounded, rendering an accuracy of a foot or less impracticable, and on account of the difficulty of recovering the same point. Pressure gauges in one form or another have been suggested, but the instrument that will sustain the load of a thousand fathoms of water and at the same time be sensitive to variations in that load of a foot or so has not yet been devised.[1]

It has occurred to me, however, that the question of tidal oscillations at sea could be approached somewhat differently, namely, by a study of the horizontal oscillations, that is, the tidal currents. In the open sea these tidal currents would, of course, be small, but not always too small to be detected and studied. Given a good knowledge of the tidal currents, the tidal rise and fall could be inferred with fair certainty. The relatively large tidal currents are to be looked for near the nodal lines of the stationary tidal oscillations, and Harris's theory will indicate plausible places in which to look for such nodal lines.

What has chiefly impressed me with the possibility of measuring tidal currents at sea was the reduction that I made for the late Dr. Harris of

[1] A recording tide gauge for work at sea invented by M. Favé, a French hydrographic engineer, that is said to have given good results at Dover, England, and in the Thames Estuary, is mentioned in the *Observatory*, 43, Aug., 1920, 279.

observations taken some thirty-five years ago by Lieut. Pillsbury,[1] as he was then, later Rear Admiral Pillsbury. They were not made with the study of tidal currents chiefly in view, but for the exploration of the Gulf Stream. Dr. Harris had them worked over again by more modern methods to see what information about tidal currents could be extracted.[2]

The series were all short, a few days at the most, and some of them did not put in evidence an unquestionable tidal current, but a number of them did. A plotting of these latter showed that the results of the approximate harmonic analyses that were made could not be far from the truth. The velocities found ranged in general from 0.05 to 0.3 knot. It may be of interest to remark that the times and directions of the current were in general agreement with Harris's theory of stationary tidal oscillations. The observations, of course, were made before this theory was formulated, but they were not reduced for tidal purposes till some time after the theory was published, so that they serve as a partial confirmation of it.

If an expedition were sent out to determine tidal currents at sea in somewhat the way here suggested, it would have to occupy one spot for several days, or preferably longer. While the vessel remained on the spot for current observations there would be an excellent opportunity for other kinds of scientific observation, magnetic, geophysical and biological. The intensity of gravity at sea is a great desideratum in geophysics and as soon as adequate apparatus is devised for the purpose observations of gravity should certainly be made in connection with observations of the currents.

EARTH TIDES AND THE VARIATION OF LATITUDE

Just as we may make a harmonic analysis of the readings of a horizontal pendulum or a Michelson tube in order to evaluate the earth tides, so for the same purpose we may make a harmonic analysis of latitude observations. In all three cases we are observing the direction of the vertical or plumb line. In the first two cases the vertical is referred to some mean position determined on the instrument itself and connected with the ground immediately around it, and thus shifting its position as the ground tilts under the influence of the tide-producing forces in the earth and under the load of the oceanic tides. In observations of the latitude the direction of the vertical is referred not to the ground round about the instrument, but to the direction of the earth's axis. The tilting of the ground is allowed for when the level readings are taken and the proper corrections for them applied. This absence of the tilting enables us to get a hold on the problem of the earth tides somewhat different from that afforded by observation with the horizontal pendulum or with the tube and interferometer.

[1] Appendices to Reports of the U. S. Coast and Geodetic Survey for 1885, 1886, 1887, 1889 and 1890. More especially that for 1890, which contains an account of the apparatus used.

[2] For the tidal data deduced see Harris's Manual of Tides, Part V (U. S. Coast and Geodetic Survey, Report for 1907, Appendix 6), 409–13.

The latitude observations most obviously appropriate for such a harmonic analysis are those of the international latitude service and I believe that they should be systematically discussed in this manner. Some preliminary work of this sort has been done by Shida and Matsuyama,[1] and Shida has proposed to the International Geodetic Association that it undertake the work. Further work on the subject has been done by Przbyllok, whose work has perhaps already been published, though without coming generally to the attention of scientists on this side of the water[2] on account of still unsettled international conditions. I believe the International Geodetic and Geophysical Union should plan to continue the work.

SUMMARY

The observation and reduction of the long-period oceanic tides should not be neglected. The equilibrium theory has not been as fully developed as is desirable and an attempt should be made to allow both for the self-attraction of the water and for the presence of the continents. When this has been done and observation compared with theory, it seems probable that a good value for the long-period earth tides will result. To get good results for the short-period earth tides the oceanic tides of like period must be known and their effects allowed for. One promising means of getting this knowledge seems to be a study of the oceanic tidal currents, which appears to be more feasible than the direct observations of the tides themselves. While the currents were being observed other scientific observations could be made. The earth tides affect the plumb line and their effects must therefore be present in the observations of the International Latitude Service. It is desirable to continue the discussion of these observations in order to throw light on the earth tides.

[1] "Memoirs of the College of Science and Engineering," Kyoto Imperial University, IV, no. 1, 1912, 277.

[2] "M. le. Prof. Przybyllok a tâché de déterminer plus exactement les constantes de quelques termes périodiques dont les observations du service international des latitudes avaient fait connaître l'existence; il s'est surtout occupé à déduire des observations astronomiques les constantes de la marée M_3 dans le mouvement de la verticale de la terre considerée comme corps élastique. Les resultats seront publiés sous peu dans les Astron. Nachrichten."—From Rapport sur les Travaux du Bureau Central de l'Association Géodésique Internationale en 1920, p. 3 (dated Jan., 1921).

SOLAR RADIATION AND TERRESTRIAL PHENOMENA

By C. G. Abbot

SOLAR RADIATION

Its Variability and Its Relations to the Atmosphere

For more than fifteen years the Astrophysical Observatory of the Smithsonian Institution has been engaged in making measurements of the radiation of the sun. These measurements have indicated that the sun's emission is variable. The Institution now maintains two stations—one in Arizona and the other in Chile—for observing the solar variability. Telegraphic reports of the results obtained in Chile have been forwarded to Buenos Aires and Rio de Janeiro for the use of the meteorological services of Argentina and Brazil.

The variation of the solar emission is of two kinds—one, of long period, associated with variations in the visible solar phenomena like sunspots, faculæ, prominences, and the like; the other, of short irregular period, apparently depending upon inequalities of radiation in different directions, which, rotating with the sun, produce at the earth the variation just mentioned. This hypothesis is confirmed by the photo-electric cell observations of Guthnick, who found variations of Saturn occurring earlier or later than corresponding ones in solar radiation observed in Chile, depending on the heliocentric longitudes of the earth and Saturn. Higher values of the solar emission occur when the sunspots are most numerous, which gives rise to a paradox, because the temperature of most meteorological stations is lower at sunspot maximum. This negative correlation between solar emission and terrestrial temperature may be due, however, to a variation in terrestrial cloudiness or to other variations in the composition of the terrestrial atmosphere. It has long been known that a close correlation exists between the sunspot numbers and the variations of terrestrial magnetism. The intermediary mechanism producing this relation is not known. However, it has long been suspected to be due to the bombardment of the earth by ions shot out from the sun. If this is the case, these ions may assist in the production of cloudiness and have also influence in the production of ozone in the higher atmosphere, and thus in one or both of these ways operate on terrestrial temperatures.

Observed variations of the sun have hitherto lain within the maximum range of about 12 percent. It is rare that fluctuations exceeding 3 percent occur within a single week or fortnight. Such studies as have been made, notably those of Mr. Clayton of Buenos Aires, indicate that corresponding to these small fluctuations of the sun there may be variations of several degrees in the mean temperature of meteorological stations. Accordingly it is highly desirable to be able to detect with certainty fluctuations of the solar emission of the order of 1 percent. This is a hard requirement, and it is only within the last year that the establishment of the Arizona

and Chile stations has warranted the hope that it can be met. Prior to that time, errors of 2 or more percent were probably not infrequent in solar radiation determinations at Mt. Wilson, which until 1918 was the only station in the world where the solar constant observations were being made. Hence we must be required to wait for another decade of years before having a thoroughly satisfactory series of solar radiation measurements to compare with meteorological observations. It would be a very great advantage if two additional solar stations could be equipped in Northern and Southern Africa in the most cloudless and favorable conditions, so that there would be four stations operating under a homogeneous scheme for determining the variability of the sun.

The question arises whether observations of the visible phenomena upon the sun's surface, such as sunspots, faculæ, prominences, or the like, or the observation of terrestrial magnetism, which, as has been said, is closely related to solar phenomena, may furnish some index to solar conditions as valuable as the difficultly obtained determinations of solar radiation. Many statistical comparisons have been published on relations of sunspots and terrestrial phenomena, and to a less degree the other solar appearances have also been correlated thereto. It must be confessed, however, that the result of this enormous amount of work has not been as favorable as would have been hoped. In almost all instances, relations which appeared to hold for a few months or years are reversed in other months or years. Except for the well-known correlation of terrestrial magnetism with the sunspot numbers, there is hardly any other pair of phenomena which would be universally accepted as related. Whether a similar disappointment will attend the proposed studies of solar radiation can not yet be foretold.

Of variable terrestrial influences, the most profoundly active on the solar radiation are the water vapor of the earth's atmosphere and the clouds and haze which are formed from it. Water vapor itself produces powerful absorption bands in the red and infra-red spectrum. Associated with dust, water vapor produces haze which is effective throughout the visible spectrum, and more effective the shorter the wave-length. A great mass of observations of these things has been made by the Smithsonian Institution in connection with its studies of the solar constant of radiation. My colleague, Mr. Fowle, has published a number of papers covering the results of these studies of the subject.

Water vapor itself removes from the direct solar beam which encircles the earth somewhere from 10 to 20 percent of its intensity, depending upon the humidity of the air and other circumstances. The haze may readily produce far greater reduction to the intensity of the solar beam. In addition, we have the clouds. My colleague, Mr. Aldrich, took advantage of the presence of the balloon school near Mt. Wilson to observe from a balloon the reflecting power of the upper surface of the vast oceans of fog which come in from the Pacific and cover the San Gabriel Valley. He found that a continuous sheet of level cloud would reflect away ap-

proximately 77 percent of the solar rays. As the earth is on the average about 50 percent cloudy, the great effect of these factors on terrestrial temperatures is obvious.

Cloud measurements are among the most unsatisfactory which are recorded by meteorologists. They depend largely on the personal equation and indeed no really adequate statistical study of them has hitherto been available. The preparation of proper automatic observing apparatus and the study of observations of clouds are highly desirable.

TERRESTRIAL RADIATION

Its Relations to the Atmosphere

When we take up the question of the terrestrial radiation, we deal with another region of wave-lengths from that which is covered by the principal incoming solar rays. The direct rays of the sun and the skylight are almost altogether confined to the region of wave-lengths extending from 0.3 micron to 3 microns. The region of the terrestrial radiation extends from 5 microns to 50 microns. Spectrum measurements have been made through a part of this region by my colleague, Mr. Fowle, who used an artificial source of light and a very long column of air of known humidity and carbon dioxide content. In this way he determined the influence of terrestrial humidity upon the rays as far as 17 microns. Beyond that, from 17 to 50 microns, no adequate studies have been made, and indeed the difficulty of making them is immense. Apparently the water vapor existing in a column of air a quarter of a mile long cuts off all terrestrial rays except in the region from 8 microns to 13 microns. In this region, water vapor is almost perfectly transmissible and in this region occurs, therefore, almost all of the terrestrial radiation which, rising from the earth's surface, escapes to space and tends to cool the earth. No constituents of the air at the earth's surface seem to affect the transmissibility of rays between 9 and 12 microns in wave-length, but the matter is quite different in the upper atmosphere. Mr. Fowle found that a strong band of absorption occurs in the direct solar beam squarely in the middle of this very transmissible region. It appears from some measurements of K. Ångstrom that the cause of this band is ozone. Thus, owing to the accidental position of this powerful absorption band in the middle of the only region where the other atmospheric constituents are almost perfectly transmissible, ozone plays an important part in determining terrestrial temperatures.

A research ought to be undertaken to determine the influence of ozone in this region of the terrestrial spectrum, the variations of its amount in the atmosphere, and, in short, the dependence of the terrestrial temperatures on ozone. This research will be very difficult, owing to the long wave-lengths of the rays involved and owing to the occurrence of ozone high up in the terrestrial atmosphere. The investigation would involve the determination of the dependence of the ozone content of the atmosphere on solar radiation as well as on influencing terrestrial conditions.

Hitherto the measurement of the outgoing terrestrial rays—that is, of the so-called nocturnal radiation—has been very unsatisfactory on account of the lack of a surface which radiates these rays perfectly. Blackened flat surfaces have been used in the instruments employed, but the blackening by means of smoke, lampblack paint, or platinum black are all unsatisfactory because these substances are not full radiators and absorbers for the very long wave-lengths involved. Smoke, for instance, is strongly transmissible beyond 10 microns, and lampblack paint falls off in its absorption very rapidly beyond 15 microns. It is necessary, in order to obtain exact knowledge, to employ some radiating and absorbing instruments which are perfectly radiating and absorbing by reason of their shape; that is to say, which approximate to the so-called absolutely black body.

Hitherto, only one such instrument has been developed, an instrument of which there is yet no published description, namely, the honeycomb pyranometer, or Melikeron, recently invented by Abbot and Aldrich. This instrument consists of 200 deep cells made by fluting a ribbon of thin manganin, the whole presenting a surface comparable to a honeycomb, in which the rays penetrate deeply and are absorbed by repeated reflections. The heat produced by the rays of the sky or outgoing to the sky can be compensated by the introduction of the energy of the electric current. This instrument is but just past its experimental stage, and only a few as yet unpublished measurements have been made with it.

Spectrum observations ought also to be undertaken in the region of wave-lengths from 15 to 50 microns. Rock salt is no longer available in this region, so that some special optical instrument, either a special grating or a special prism to be made of potassium iodide, must be employed.

NEEDED INVESTIGATIONS

It will be seen from these remarks that the most outstanding needs in the investigation of radiation for meteorological purposes are:

First, the continuance of accurate observations of the variation of the sun. These measurements are now going on under the auspices of the Smithsonian Institution in Arizona and Chile, but should preferably be supplemented by the provision of two additional stations, perhaps in North and South Africa, so that variations of the sun could be adequately studied every day in the year. Twenty-seven thousand dollars a year would provide the two stations within two years and maintain them thereafter perpetually.

Second, the painstaking studies of terrestrial cloudiness, its causes and its effect on the incoming of solar radiation.

Third, the study of the quantity and variability of ozone in the upper atmosphere, its dependence on solar and terrestrial conditions, and its influence on terrestrial temperatures.

Fourth, extensive studies with the "black-body" nocturnal radiation

instrument, and if possible the development of new instruments of that class.

Fifth, an investigation of the effects of terrestrial atmospheric constituents on rays between the wave-lengths of 15 and 50 microns should be undertaken. This involves the development of special optical means to take the place of rock-salt prism as a dispersing medium, since rock salt is non-transmissible to the rays in question.

RELATIONS BETWEEN SOLAR ACTIVITY AND ITS VARIOUS ASPECTS AND THE PHENOMENA OF TERRESTRIAL WEATHER

By C. F. Marvin

My contribution to this discussion is an appeal for a more careful and consistent recognition of the effects and operations of chance in the study of data which may be employed in investigations of solar and terrestrial relations, periodicities, etc. Weather conditions, atmospheric transmission of radiation, magnetic phenomena, sunspots, observed intensities of radiation, and values of like phenomena are subject to large and irregular accidental variations, due account of which must be taken in reaching conclusions.

SOLAR RADIATION

My point of view is illustrated in an admirable manner by figure 1, which serves to show how weak the observational basis still is to justify the claim that there are important irregular changes from day to day in the intensity of solar radiation.

Discussion of diagram.—The vertical lines of the diagram represent throughout each group 25 observations, counted from the beginning of the group. The points on the zigzag lines are simply consecutive observations without reference to the interval of time between observations.

The chronological sequence of the groups of data is represented by the numbers 1, 2, 3, etc. Only a few observations were made between 1902 and 1907 at Washington at wide intervals. From 1905 to 1918 (groups 2 to 7, inclusive) the observations were made at Mount Wilson, Calif. In all these cases the interval between observations is as nearly as possible one day, although periods of bad weather frequently caused two or three, or more days sometimes, to intervene. As a rule, observations were made only from June to November.

Extreme variability is shown in the observations in group 1. Groups 2, 3, 4, 5, and the first portion of 6, show distinctly a lower order of variability, although occasional extremely high and low values occur infrequently.

Beginning at K, group 6 for 1912, great variability again appears in the consecutive values extending into, although diminishing during, 1913. The high value at K, group 6, marks the arrival at Mount Wilson, Calif., of the dust from the Katmai volcanic eruption.

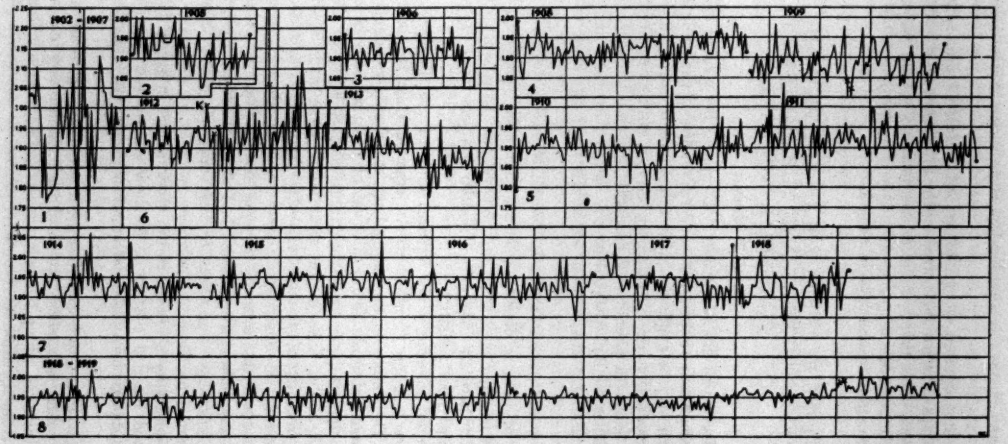

FIG. 1. Diagram of nearly 2000 consecutive observed values of extra-atmospheric intensity of solar radiation, as determined by the Smithsonian Institution from the beginning of observations 1902 to the end of 1919, at stations, Washington, D. C., Mount Wilson, Calif., and Calama, Chile.

From 1914 to 1918, well after the atmosphere had cleared itself of the Katmai dust, the variability became quite similar to observational results at Mount Wilson prior to the Katmai year, 1912.

Group 8 represents the observations at the station at Calama, Chile.

All observations, including the first portion of group 8, were made by the bolographic method.

Finally, the last group of observations, ending December 31, 1919, were made by a new empirical method based on the bolographic method but permitting two or three observations to be made the same day, thus giving a mean average value for a day of higher accuracy.

This diagram tells a very important story with great force and plainness. Great variations in consecutive values of intensity mark the early observations in Washington with imperfect equipment and poor atmospheric conditions.

Observations at Mount Wilson from 1905 to 1912 show far more nearly constant values of radiation until the arrival of the atmospheric dust from the Katmai volcanic eruption, after which day-to-day or consecutive values showed great variations. Everyone probably ascribes these increased variations, not to increased solar activity, but to inaccuracies of measurement due to atmospheric dust. The large variations disappeared with the dust. Furthermore, some increased accuracy (smaller variations) characterized the observations at the station at Calama, Chile, either because of the better instrumentation, greater observational experience, or better observing conditions, or all of these in combination.

Finally, it is most striking that a further marked reduction in day-to-day variability immediately resulted from the introduction in 1919 of the pyranometer method of observation.

The percentage *probable error of a single value* has been carefully computed for each group of observations and the results are shown graphically in figure 2.

Entirely terrestrial causes easily explain the great changes and gradual diminution in variability shown by the observations, the accuracy of which has been wonderfully increased by improvements in instruments, methods, and location of stations. What are now regarded as good observations *for a single day's work* show a probable error as low as 5 or 6 tenths of one percent. This is remarkable precision. This analysis of the whole body of radiation data brings one face to face with the important question:

Is all of this 5 to 6 tenths of one percent of day-to-day variation in solar radiation intensities real error of measurement only? Or is part of it error of measurement and part real solar change? If the latter, what are the respective amounts of each variation?

Some conclusive answer to this question is necessary before inferences and claims of solar and terrestrial correlations can be set up and justified.

It can never be claimed, of course, that single daily values, however carefully made, are perfectly accurate. Probably simultaneous observa-

tions at *several* stations is the only answer to this question. Caution is necessary even here, because mere coincidence of variations due absolutely to errors only will come in to affect comparisons at two stations. If, for example, e is the probable variation of, say, a season's work at two per-

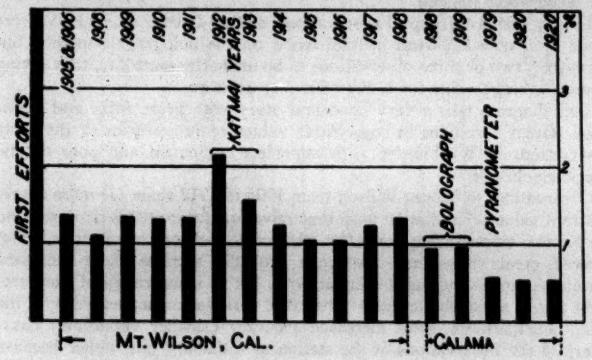

FIG. 2. Height of bars shows probable error of an observation for a single day of the intensity of solar radiation as measured by the Astrophysical Observatory of the Smithsonian Institution at Washington, Mount Wilson, Calif., and Calama, Chile.

fectly equal stations, then for pure chance relations between values the variation ϵ_m of the mean of the two must be

$$\epsilon_m = \frac{\epsilon}{\sqrt{2}}$$

This furnishes an interesting test to apply to the simultaneous observations at Arizona and Chile when these are released by the Smithsonian Institution.

TERRESTRIAL DATA

No serious study of any kind dealing with hidden or obscure relations between data subject to large irregular variations should ever be undertaken without a careful application of the principles of probabilities and a consideration of the operation of the elements of chance upon the phenomena under study.

In the discussion of this portion of the subject the author gave the first

public account of the mathematical and graphic device which has been designated *The Periodocrite.*

Periodocrite[1] is a word coined from Greek roots signifying a critic, a judge, a decider of periodicities, and is a name applied to a mathematical and graphic method or device which has been developed to aid in the conclusive separation of obscure and hidden cycles and periodicities possessing a real existence from those whose essential features are only such

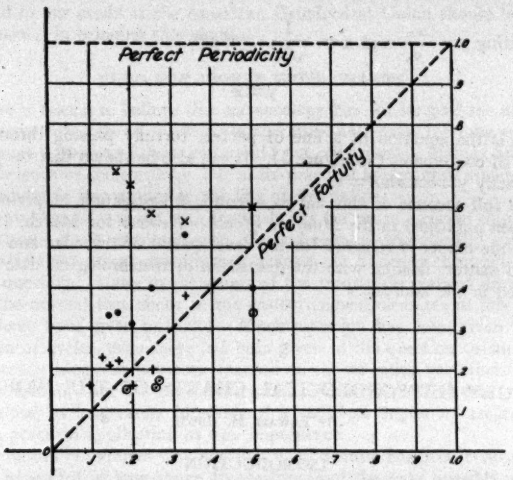

FIG. 3. Rainfall periodocrite: ×, Annual cycle five stations in Iowa, 36-year record; ●, Annual cycle Washington rainfall, 50-year record; +, Annual cycle Boston, Mass., 103-year record, very feebly defined; ⊖, A 15-month sequence Iowa rainfall; other sequences, 15 months, 16 months, one-ninth the variable sunspot period, like the circles, all fall in the class of perfect fortuity.

as would result from, and can be explained by, entirely chance combinations of the data employed.

The periodocrite does not disclose or discover the *length* of suspected periods or cycles. Other methods, such as the harmonic analysis, Schuster's periodogram, or any of the many methods which have been offered for this purpose must first be employed to ascertain the proper length of any suspected cycle.

[1] Prof. C. F. Talman supplied this name from Κεριοδος, a period + Κριτης, a judge, decider, umpire, from Κρινα, to separate, investigate, judge.

The theory of the periodocrite depends upon the principle that in an entirely fortuitous combination of data the standard deviation, σ_n of a result made up of n individual observations in combination is given by the equation $\sigma_n = \frac{\sigma_o}{\sqrt{n}}$ in which σ_o is the standard deviation of the original data not in combinations.

Writing $y = \frac{\sigma_n}{\sigma_o}$ and $x = \frac{1}{\sqrt{n}}$ we get

$$y = x$$

which is the equation of a line of perfect fortuity passing through the origin of coordinates (see figure 3). It can also be shown that for perfect periodicity $y = \text{constant} = 1$.

The full account of this aid to scientific investigation of periodicities has been published in the *Monthly Weather Review* for March, 1921, to which the reader is referred for the development of this idea and certain related matters dealing with the discussion of meteorological data briefly outlined in the address.

DAILY METEOROLOGICAL CHARTS OF THE WORLD

By Edward H. Bowie

INTRODUCTION

Investigations in the field of general meteorology have been and continue to be restricted and handicapped by the fact that there are not available to investigators daily charts of the world's weather. It is necessary in investigations in meteorology and particularly in weather forecasting that problems now but imperfectly understood be considered from a world-wide viewpoint, for there can be no doubt that much that we call "weather" is not of local origin, but has its inception in the general actions and reactions that involve at times the atmosphere over an entire hemisphere and possibly both hemispheres. Hence, investigation based on a study of daily synoptic charts for a limited area, such as Europe, the United States of North America, or of India, can lead to but an imperfect understanding of the general physical processes that are in operation to produce our day-to-day weather. Moreover, when it is understood how rapid are the changes in speed, direction of movement and magnitude of

areas of high and low barometric pressure, there arises the natural desire to look into the observable facts over a world-wide area in an attempt to determine the causes of them. Meteorology without a world-wide weather map is laboring under difficulties as great, or greater, without realizing it, than astronomy without its star charts. Hence it is contended that many of the important problems of meteorology will not and cannot be solved until there be available daily synoptic charts of the various meteorological elements of, relatively speaking, the entire world. It would redound to our credit if the American Geophysical Union should become instrumental in bringing this about.

NEED FOR WORLD-WIDE CHARTS

There is reason to believe that meteorology has for its goal the making of accurate forecasts of weather, temperature and wind for long periods in advance. Such an attainment would not only mark a distinct advance in the science of meteorology, but in its practical application would be of great economic importance to the life of the nation.

The problem of making such forecasts has for years received consideration not only from meteorologists of good repute—scientifically speaking—but also from others untrained or at least not having a thorough grasp of the question. Many explanations of the frequently marked deviations from the normal that occur in the meteorological elements at any given place have been given publicity. Much attention has been given to the question of cycles, even more has been given to the question of sunspots, and recently attention has been focused on the so-called variations in the solar constant. But all these efforts fail to offer a method that permits the making of long-range forecasts of a sufficient degree of accuracy to have a practical application of any importance.

One naturally inquires why nothing really definite has been evolved that might be useful in long-range forecasting, and the only possible answer that can be made is that the right combination has not yet been found.

My impression is that the answer will probably be had from a prolonged and careful study of world-wide meteorological conditions, and this study is possible only in the event that daily synoptic charts embracing the major part of the world are available. In my opinion all marked deviations from normal weather and temperature are associated with changes in magnitude and position of the so-called "great centers of action." Any material addition to our knowledge of these must come from the study of world charts, for it is only by such a method that we can hope to gain a more comprehensive knowledge of the general atmospheric circulation and the resulting changes in the centers of action. In Supplement No. 1, *Monthly Weather Review*, 1914 (Bowie and Weightman), it is stated that:

> Conspicuously abnormal pressures in the regions of these so-called "centers of action" are related to marked departures from normal weather and temperature conditions in the United States. Some authorities assume that these abnormal distributions of pressure are due to extra-terrestrial and others assert that they are due to terrestrial causes. If it be true that the solar output is a variable quantity,

it is possible that the solar variations are associated with marked changes in pressure in the "centers of action," and thus may be found a key for defining for considerable periods in advance the general character of coming weather changes for a given region. If on the other hand abnormal pressure distributions occur with an unvarying solar radiation, the causes thereof must be traced to a terrestrial source. The varying effects of the nearly constant radiation on land and water surfaces and on the air under different conditions of temperature, water vapor content, dust content, etc., are sufficient in the minds of some writers to explain these phenomena, i. e., the changes in the position and magnitude of the "centers of action."

Regardless, however, of the cause of abnormalities in the "centers of action" the importance of their relation to the character and paths of storms in the United States is well recognized and therefore should be carefully considered in day-to-day weather forecasting in the United States. To illustrate: Of the centers of action that affect the weather conditions of the United States east of the Rocky Mountains, the subpermanent high over the middle latitudes of the North Atlantic Ocean is perhaps the most influential. When this is well developed and stable, temperatures above the seasonal average are to be expected over the great central valleys and the eastern and southern states, and the areas of high and low barometer crossing the United States will move in high latitudes and pass on to the ocean by way of the St. Lawrence valley. In fact, all prolonged periods of heat in the regions east of the Rocky Mountains occur simultaneously with the abnormal development of this subpermanent high. When, however, it is weak and ill-defined, cool weather prevails over the eastern half of the country.

Again, the variations in the position and magnitude of the elongated subpermanent area of low pressure that normally extends from southeastern Alaska westward to Kamchatka, have a decided influence on the characters of, and courses followed by, storms that cross the United States. If this Aleutian low is north of its normal position, lows will move along our northern border; whereas, if it is south of its normal position, lows will move far south of their normal tracks and stormy weather with great alternations in temperature will occur over the United States.

Perhaps the best examples of unusual winters in the United States are those of 1917-1918 and 1920-1921. In the former, which was one of great severity, the pressure was abnormally high over Alaska and the Aleutian Islands; while in the latter, which will go down in meteorological history as one of the mildest known, the pressure was much below the normal over those regions.

To be able to formulate correct forecasts, a knowledge of the general circulation is fundamental. It is questionable whether a proper understanding of the general circulation can be gained from monthly averages. It certainly is not as stable as the text books would lead us to believe, for there are frequently marked changes in both the surface and upper air flow. It has been customary to think of the general or primary wind circulations of the two hemispheres as separate and distinct, but this view is not tenable. A cursory examination of plate 14, "Bartholomew's Physical Atlas," Meteorology, volume III, shows a tremendous seasonal interflow between the northern and southern hemispheres, indicating that the two systems of general circulation are in a way interlocked. In the winter of the northern hemisphere the air flows normally from the interior of Asia southward over the Indian Ocean, eastern Africa and the East Indian Archipelago on beyond the equator as far south as northern Australia; in the summer of the northern hemisphere a return flow takes place over essentially the same geographical area. No one can say, for the lack of the necessary data, whether these currents do not bring about

profound changes from normal weather and temperature conditions over large areas outside the regions where these flows and counterflows are in operation, but it seems logical to suppose that such is the case. Further, the trades and antitrades are probably not fully understood. Certainly these wind systems undergo pronounced changes that are independent of the seasonal changes. In connection with the antitrades, Sir Napier Shaw in a recent number of *Nature* remarks:

> At the same time I may remark that I find it very difficult to grasp the meaning that is intended by "anti-trades." The original convection theory suggested that the anti-trade was the trade returning up aloft above its old path, but, so far as I can understand the situation, the track of the wind from the equator must begin from the east and become southwest by what I will describe as the hurricane track. On the other hand, a southwest wind may be a part of the westerly circulation diverted; the difference of origin of the observed southwesterly wind is of some dynamical importance.

It seems possible that the antitrade may be a northward extension of the southeast trade of the southern hemisphere, which on crossing the equator is turned to the right by the deflective force of the earth's rotation, and being warmer and of less density, overruns the low-lying northeast trade. But this is not definitely known and sufficient data are not at hand to prove or disprove the assertion.

Bjerknes has recently given meteorology the term "polar front," a line of discontinuity separating the polarward from the equatorward flowing winds, and he has urged the collection of daily meteorological observations from larger geographical areas that this "polar front" may be delineated on the weather charts for the aid of the forecaster. He believes this essential to forecasting, for the theory he advances places the origin of cyclones and consequently all marked variations in weather, temperature and wind changes along this line of discontinuity. His presentation of the idea of the "polar front" and its attendant phenomena is worthy of him, and augments the necessity of observations over large geographical areas in weather forecasting.

It will be seen from the foregoing that meteorology must in the near future consider the question of securing observations from every accessible place and assembling them for the construction of daily world-wide weather charts at one or more great world centers for intensive study. First, there must be a skeletonized chart based on observations collected by cable, radio and land lines, and, second, a more nearly perfect and complete chart based on the same observations supplemented by those collected from remote land areas in which cable or radio is not available and from ships at sea. The former chart would serve for day-to-day forecasting; the latter, for study purposes and eventually for long-range or seasonal forecasting.

HISTORY OF THE MOVEMENT FOR WORLD-WIDE CHARTS

The need of a daily synoptic survey of the earth's atmosphere was coincident no doubt with the beginning of synoptic weather charts, which was at approximately the middle of the nineteenth century. We learn

that at the first meeting of the International Meteorological Congress, assembled at Vienna in 1873, a proposition was adopted to the effect:

> That it is desirable, with a view to their exchange, that at least one uniform observation, of such a character as to be suitable for the preparation of synoptic charts, be taken and recorded daily and simultaneously throughout the world.

Later, on December 9, 1876, it was announced that the United States, through the Chief Signal Officer, U. S. A., no doubt inspired by the late Prof. Cleveland Abbe, then assistant to the Chief Signal Officer, was undertaking the task of establishing cooperation for the recording and exchange of simultaneous meteorological observations between the United States and the following named countries: Algeria, Austria, Belgium, Great Britain, Denmark, France, Germany, Italy, The Netherlands, Norway, Sweden, Switzerland, Turkey, Greece, Canada, the Hawaiian Islands, Dutch Guiana and Japan. This cooperation extended to naval and merchant vessels of these nations, and thus were secured the simultaneous observations of atmospheric changes over much of the northern hemisphere. Thus came about the "Bulletin of the International Meteorological Observations," published by the Signal Service, U. S. A., for the years 1877–1887—incomplete, it is true, as to world-wide weather maps, but a remarkable contribution which has left its impress on meteorology even until today.

An effort to accomplish the preparation of northern hemisphere weather maps by means of daily telegraphic reports for the purpose of extending the forecast period to cover the general weather of the United States was undertaken by the U. S. Weather Bureau in 1907, and the area covered by such reports grew until the outbreak of the Great War in 1914, when the scheme was unavoidedly interrupted. No other really effective efforts looking to the preparation and publication of even partial world-wide daily synoptic charts are known to me.

PRESENT STATUS OF DAILY SYNOPTIC METEOROLOGICAL CHARTS

Nothing approaching a world-wide daily synoptic chart is prepared and published by the meteorological service of any nation. Instead, every, or nearly every, national meteorological service decides for itself: (a) the scale of the map, (b) the units of measurement, and (c) to a greater or less extent the hours of observation. In addition to the various charts of land observations, charts are also made of the meteorological conditions over one or more of the oceans. Daily weather maps for their respective geographic areas are now prepared and published by the United States, Canada, Mexico, Argentina, Chile, Brazil, Japan, China (Zi-ka-wei Observatory), Australia, India, South Africa, Great Britain, France, Portugal, Belgium, The Netherlands, Norway, Sweden, Denmark, Germany, Austria (?), Russia (?) and others. There is thus available a tremendous mass of valuable data awaiting action that will assemble them into one standard, world-wide weather map which will permit investigations in meteorology to be carried beyond any point now possible.

RECOMMENDATIONS

This matter is believed to be of such importance at the present time that it is contemplated recommending in appropriate form that some action be taken by the Geophysical Union for the accomplishment of the desired objects through international cooperation.

There is no doubt in the minds of those familiar with the present status of meteorology that the carrying out of this proposal will be well worth while, not only from a scientific standpoint but also from the standpoint of service to the general public.

The preparation of the data for charting and the printing or lithographing of the charts for American use could best be done by the U. S. Weather Bureau, but to do this, additional funds must be provided through congressional appropriation.

WORLD AEROLOGY

By Willis Ray Gregg

Aerology may be very simply defined as "the study of the free air";[1] world aerology, as an extension of that study to all parts of the world. By this we mean not only the continental areas, but the seas as well; and not merely sections of a hemisphere, but from pole to pole. It is our purpose to review very briefly (1) what has been and what is being done toward this end; (2) more particularly, to outline what *can at once* and also what *should* later from time to time be undertaken.

PAST AND PRESENT

Methods.—As early as the middle of the 18th century kites were used, by William Wilson at Glasgow University and by Benjamin Franklin at Philadelphia, in making free-air observations. Others followed their example, with more or less success, but it was not until about 1890 that the kite came into general use for this purpose.

So far as known, the first manned balloon ascent in the interests of science was made by Robertson and Lhöest in 1803. During the next 75 years much interesting information as to free-air conditions was obtained by means of numerous similar ascents, among the most notable of which were the classic voyages of Glaisher, Flammarion, de Fonvielle and Tissandier.[2] Unlike the kite, however, the manned balloon has in recent years suffered a decline as a means of aerological exploration, because of the large expense involved and the impossibility of providing satisfactory exposure of the instruments. Although the observations made by these two methods were extremely interesting, yet prior to 1890 they yielded comparatively little of value, owing to their fragmentary character and none too great accuracy.

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[1] See "Meteorological Glossary," British Meteorological Office, M. O. 225 ii, 1918, p. 16. London.

[2] "Travels in the Air," edited by James Glaisher, F. R. S., Philadelphia, 1871.

Since 1890 rapid strides have been made. The kite has been developed from a mere toy into a very efficient means of exploration. With it heights slightly exceeding 7 kilometers have been reached, although the average daily height is a little under 3 kilometers. Recording instruments carried by these kites furnish information as to pressure, temperature, humidity and wind at various heights and their changes from day to day, season to season, and under various types of weather at the earth's surface. Since the kite can be flown only when there is appreciable air movement, its use has in some instances been supplemented by that of a small captive balloon, and thus we have some records in calm weather. Generally speaking, however, the captive balloon has proved to be rather unsatisfactory and its use has been largely discontinued.

For exploring the air to greater heights than can be reached with kites, so-called "sounding" balloons are used. Made of pure rubber, filled with hydrogen and carrying self-recording instruments, these balloons have given us information of great interest and value to heights of 30 kilometers or more. Smaller, so-called "pilot" balloons, because of their comparative cheapness and convenience in handling, have in recent years come into general use for observing wind direction and speed. On clear days, when the wind is not too strong, these balloons can be followed by means of theodolites to heights well above 10 kilometers.

All of these methods have been rather extensively employed in Europe, particularly in England, France and Germany, and in the central and eastern portions of the United States. Some of them have been used to a limited extent also in Canada, Australia, Java and Argentina, as well as on a few expeditions of short duration to different parts of the Atlantic Ocean. In addition, there should be mentioned the great mass of cloud observations, some of which, particularly those during the International Campaign of 1896–97, were accurately and systematically made by means of nephoscopes and theodolites and furnished information, not only as to the heights and other characteristics of the clouds themselves, but also as to wind conditions at various levels.

Results.—Although a considerable amount of data has been gathered by the methods above outlined, it must be confessed that we know even yet comparatively little with reference to what is going on in the atmosphere above the earth's surface. The general state of our knowledge can be briefly summarized as follows:

(a) For parts of Europe and the United States we have well established average monthly, seasonal and annual values of all the meteorological elements from the surface to about the 5-kilometer level. Pressure of course always diminishes with altitude; temperature and humidity do so on the average, except that in the north-central portions of the United States there is a temperature inversion in the lower levels during the winter months; wind velocity increases, sharply in the lowest half kilometer, more gradually above that height; and wind direction is in the mean very nearly westerly at all levels, except in the southern part of the United States, where, during the summer, it is south to east near the surface.

(b) Of conditions between 5 and 25 to 30 kilometers we have rather limited information. We know that the temperature continues to diminish at a fairly uniform rate until a height of 8 to 18 kilometers is reached—this height varying with latitude, season, and sea-level pressure; above this limiting height the temperature ceases to diminish and in fact has a tendency to increase to some extent, at any rate during the summer half of the year. The boundary plane between the lower region of temperature decrease, known as the troposphere, and the upper region of little temperature change, known as the stratosphere, is in general well defined. Clouds do not occur in the stratosphere and winds generally have lower speeds here than in the troposphere. There is some evidence that at still greater heights wind direction changes from westerly to easterly, but data on this point are not conclusive. Other characteristics of the stratosphere are the lower temperature and greater height of its base in low than in high latitudes and during falling than during rising air pressure at the earth's surface; also, its greater height in summer than in winter.

(c) Of the relations found to exist between surface weather and free-air conditions, perhaps none is more significant than that between surface temperature distribution and winds in the upper levels. As is well known, the winds at and very near the surface conform quite closely to the surface pressure gradient, but at greater heights they often depart widely from it. If the temperature is fairly uniform over wide areas, the free-air winds are very nearly parallel to the surface isobars, and show that anti-cyclones and cyclones extend as such to great heights. If, on the other hand, the latitudinal temperature gradients are steep at the surface and also, though to a less extent, in the higher levels, then the surface pressure systems lose their identity at a very low altitude, the isobars opening out on the north side of cyclones and on the south side of anti-cyclones, and the winds veering or backing from those at the surface in conformity with the altered pressure distribution at the higher levels. This relation of free-air winds to surface temperature distribution has not thus far been accorded the attention it deserves. With the development of aviation and the resulting demand for accurate free-air wind forecasts, the significance of this relation must necessarily receive increasing recognition.

The foregoing summary is very sketchy and incomplete, but it will serve as a basis for the consideration of problems which must be attacked in the future, if real progress is to be made.

THE FUTURE

The present age, to a greater extent than any in the past, may be called an "age of projects." More and more mankind is giving heed to Emerson's exhortation, "Hitch your wagon to a star," and perhaps this is an especially appropriate motto for the aerologist to adopt as his own. Of the many ambitious plans that we read and hear about, some undoubtedly will yield negative results only, but it is equally certain that others will contribute very materially to human welfare. And it is better that some

should fail than that none should be tried. The projects to be presented here are not visionary, but on the other hand very practical ones, and for the most part they are not difficult to carry out. They will be stated in the order in which it is believed they can be put into execution.

1. *Further study of data already accumulated.*—There is much material now available that has not been summarized and studied in detail or properly applied to the problems of aviation and forecasting, and to the solution of perplexing questions relative to the larger features of atmospheric circulation. One of the first things to be undertaken is the preparation of such a summary. Very few men, outside of the government services, are giving the subject any thought. Those *in* the government services can devote comparatively little time to it, because of other more pressing duties. There are needed at once for this purpose half a dozen well-trained men (well trained both in theory and in field experience) who can give all of their time to this subject for a period of 3 or 4 years. This, then, is a comparatively simple project—one requiring only a small outlay of funds, but giving results of immense value.

2. *Development of new methods of observation.*—As already stated, nearly all observing at the present time is done with nephoscopes, kites, pilot and sounding balloons. All of these methods have well-known limitations, but should be continued. There should be added, if possible, observations with kite balloons and airplanes. Kite balloons, although more expensive than kites and pilot balloons, would furnish data of correspondingly greater value, since they could be used with greater regularity, irrespective of weather conditions. Indeed, their use would be limited only by very high winds, and records could thus be obtained under conditions unfavorable for kites; in other words, by combining the two methods, the atmosphere could be explored up to 3 kilometers practically every day in the year. Like kites, their use would be restricted to regions not frequented by airplanes, because of the danger of fouling with the cable. Their use would be further restricted to places where hydrogen could be obtained. These limitations, however, are no more serious than others under which we now labor and can be overcome at comparatively small expense, when we consider the great value of the results obtained.

Development of suitable apparatus for use in airplanes should be pushed vigorously. As aviation expands, there will necessarily be a large number of places at which regular daily flights will be made at and above the fields for purposes of testing the machines and the training of pilots. Observations during these flights would add little expense and would provide information not otherwise obtainable, such as the thickness of cloud layers, etc. Work along this line has been done in England and France, but thus far to no great extent in this country, because of inadequate appropriations. It will be taken up as soon as funds for the purpose are made available.

3. *Extension of observation stations to all parts of the world.*—This must be done through international cooperation, but the United States can

make a good start by more completely covering its own territory, including Alaska, the Hawaiian and Philippine Islands, etc. In making such an extension, and a further extension later by all other countries, two separate and distinct purposes are to be served: (a) the furnishing of current information of immediate practical value to aviators; (b) the

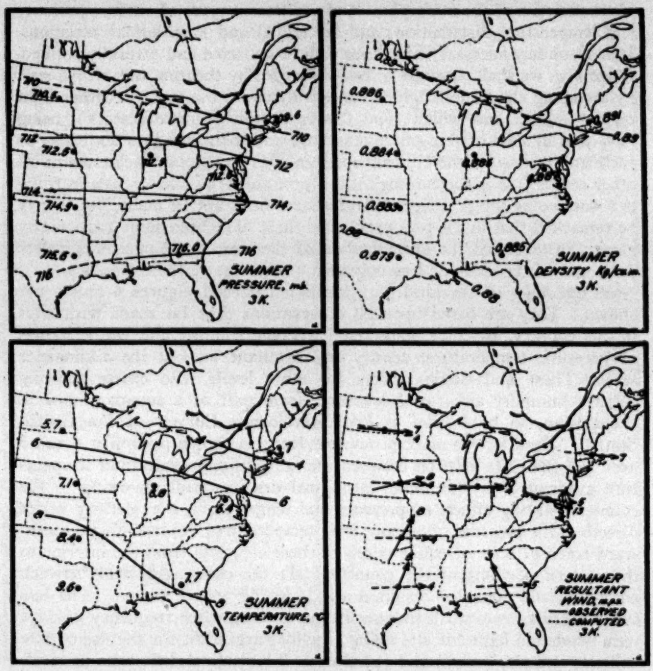

FIG. 4. Average summer values of pressure, temperature, density and resultant wind at the 3-kilometer level.

collection of statistical information required to explain the physical causes of various phenomena and, as a necessary consequence, to increase the accuracy of weather forecasting. For the first purpose it is sufficient to establish observing stations having comparatively simple equipment, by means of which the atmosphere may be explored to moderate heights only. It is not essential that temperature and humidity be observed, but it *is* essential that frequent observations be made of wind, cloudiness and visi-

bility, these being the factors of vital interest to aviators. For the second purpose a much more comprehensive program is necessary. We should have accurate values of temperature and moisture as well as of wind. Observations should extend to as great heights and be as nearly continuous as possible, in order that we may know the diurnal and annual variations throughout the troposphere and much of the stratosphere; the characteristics of the atmosphere under different types of surface pressure and temperature distribution; and latitudinal and longitudinal variations. It is absolutely necessary that these data be collected and carefully studied. Otherwise we shall continue to be bombarded by theories and, worse still, by sweeping conclusions which can hardly stand the test of further light on the subject, but which (and this is the unfortunate feature), being advanced by men of recognized standing, find their way into textbooks as *facts* and thus start the student upon an entirely wrong track. As in all other matters, so in meteorology it is regrettably true that "a little learning is a dangerous thing." Specific references need not be made, but it may be remarked that in the past year or so there have been some particularly glaring instances of the promulgation of theories, based upon incomplete data, and of the more or less universal acceptance of those theories.

As examples of the kind of information needed, figures 4 and 5 are shown. They are based upon all observations thus far made with kites in this country, and give respectively average summer and winter values of pressure, temperature, density and resultant wind at the 3-kilometer level. These and similar charts for other levels, also charts showing relative humidity and vapor pressure, form part of a summary now in preparation, to be known as "An Aerological Survey of the United States." Some of the more prominent features shown in figures 4 and 5 are: (a) the close relation between the latitudinal pressure and temperature gradients; (b) the small latitudinal density gradient, owing to the counterbalancing effects of pressure and temperature, i. e., density varies directly with pressure, inversely with temperature; (c) the slight southward trend of lines of equal values of these elements from the interior to the eastern portions of the country; (d) the close agreement between computed and observed resultant winds in the winter season. The less satisfactory agreement in summer is due to the greater frequency of days with winds too light for kite flying (another argument for the use of kite balloons and airplanes); and (e) the small latitudinal difference in resultant wind speeds, due to the fact that these vary directly with the pressure gradient, but *inversely* with the sine of the latitude.

It is probably not a coincidence, but rather a matter of considerable significance, that the average movement of cyclones in the United States during the winter, as determined by Bowie and Weightman, is 13.4 m.p.s.[1] —a value in striking agreement with the resultant wind shown in figure 5. The agreement is less close in summer, apparently indicating that cyclones

[1] Types of storms of the United States and their average movements. *Monthly Weather Review*, Supplement no. 1, p. 8.

extend to a greater height in that season than in winter, and this, as already pointed out, is undoubtedly the case.

These figures, however, are not shown with the view of discussing them as such, but rather with that of indicating how important it is that we obtain similar information for all other parts of the world—the sea as

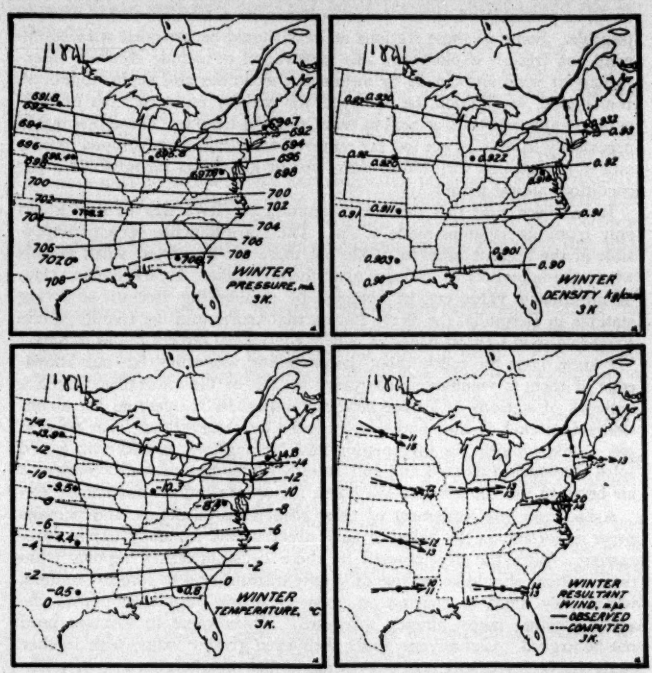

FIG. 5. Average winter values of pressure, temperature, density and resultant wind at the 3-kilometer level.

well as the land. Obviously, it is impossible to carry out this program at once in its entirety. We must therefore start with the most pressing needs, as follows:

It is well known that the type of pressure distribution prevailing in the region of Alaska exercises a dominating influence on the weather of the United States. Similar relations are found in other parts of the northern

hemisphere and emphasize the importance of having a network of stations, observations from which would make possible the construction of world weather maps—a subject which has already been presented by Major Bowie (see page 36). As indicated by him, such observations would enable the forecaster to follow from day to day the eastward march of the so-called "polar front."[1] There should be a string of stations as far north as possible and, in the southern hemisphere, another as far south as possible. Some of these stations at least should be provided with equipment for free-air exploration, this exploration to include accurate observations of wind and clouds by means of theodolites and nephoscopes. A few should, in addition, be equipped for making measurements of temperature and moisture, as well as wind, to great heights. These few would necessarily have to be not too far removed from sources of supply, but the others, if equipped with radio, could well be located as far north as living conditions would permit.

It has been said that definite meteorological laws will be established only from observations made at sea. These are difficult, perhaps impossible at the present time, to make, but there are numerous small islands where the influences of the land upon the atmosphere are negligible. Data of inestimable value can be obtained by establishing free-air observing stations in Bermuda, the West Indies, the Azores and the islands of the Pacific; also in Central America, where continental effects would be small. We know none too much about the prevailing westerlies, but our knowledge of them is voluminous compared to that of the antitrades. Such a network of stations as I have indicated, especially if operated for a considerable period of time and supplemented by observations from ships at sea, would provide the information now lacking and, in addition, would solve the much discussed and still unsettled questions of the exchange of air between the equator and the poles, the movements of hurricanes, etc.

Aside from the settlement of these theoretical questions, and perhaps more important, is the value of such observations for daily use in forecasting. With the development of radio communication, reports from these stations should be capable of speedy transmission to forecast centers, where they could be charted on upper-air maps, supplementary to the world weather maps, already discussed. Their value to aviators need not be argued. Can anyone doubt their even greater value, with further study, to the forecasting, not only of day-to-day weather, but also of week-to-week, month-to-month and possibly year-to-year changes in weather?

[1] See V. Bjerknes. The meteorology of the temperate zone and the general atmospheric circulation. *Nature*, June 24, 1920, 522–524.

WORLD DIGEST OF METEOROLOGICAL DATA

By W. J. Humphreys

Meteorological data are gathered for, and serve, many purposes:

They are abundantly used in forecasting the weather of the morrow, but obviously used only once, and hence for this purpose need not be recorded.

They also are collected in the course of special studies, but the completion of each investigation renders useless the preservation of the particular material treated. It is the generalization—the law—that counts, and not the isolated values from which it happened to be deduced.

Finally, they are essential to many studies of interrelations between meteorological elements; to a knowledge of the relation of the weather in one part of the world to that occurring either previously, simultaneously, or subsequently, in others; and to all accurate knowledge of climates and their changes. For each of these purposes it is necessary that meteorological data be indefinitely accumulated, and equally necessary that they be put in manageable form and made widely available.

Now, although fully three-fourths of the surface of the earth is a meteorological blank, the mass of data already accumulated from the remaining one-fourth is so vast and heterogeneous as to be beyond the power of any individual to analyze and study in detail. Furthermore, even approximately complete sets of these data have been assembled in very few places. Hence much of the information contained in this meteorological material certainly is not only unknown, but even beyond the power of individual effort to know.

Therefore it is suggested that a comprehensive digest of all existing meteorological data be made and published. A possible way of accomplishing this great labor is as follows:

1. Let the data to be published (monthly and annual normals and departures therefrom, special phenomena, and what not), the units to be used, the form of publication, and all other details of this kind, be agreed to internationally.
2. Let each country furnish the digest of its own data.
3. Let the digest for each country consist of the individual digests for, and made at, its several meteorological stations.
4. Let some one agency, supplied with adequate funds and personnel, be charged with the duty of assembling sporadic data from countries that have no official meteorological organization; and with the further important duty of editing the entire work.

In this way the proposed vast labor would be divided up between several countries, and further subdivided among many individuals in each country, and the product of the combined effort of the many workers—the digest of all the world's meteorological data—soon made available to every institution that needs it and to every individual who wishes to study it.

There then could be students of world meteorological data, and would be; now there is none—and can not be.

It will be recognized of course that the plea here is for a greater "Réseau Mondial," one covering a larger number of meteorological elements than does that splendid publication, and also extending back to the beginning of meteorological observations. It would both include and supplement the data contained in the present Réseau Mondial, but would not take the latter's place as a convenient annual summary of the more important elements of the world's weather at selected places.

GENERAL ADOPTION OF THE CENTESIMAL SYSTEM OF ANGULAR MEASUREMENT—WITH APPLICATION TO ANEMOMETERS AND NEPHOSCOPES

By Alexander McAdie

Reviewing an article on "Uniformity in Aerographic Notation,"[1] Sir Napier Shaw[2] calls attention to the common usage of the capital letters N.E.S.W. for wind directions, and the established usage in Physics of N for Avogadro's constant, E for Energy, S for Entropy and W for internal work.

The criticism is constructive and suggestive. The question arises: Is it not desirable to follow the lead of navigator and magnetician and use degrees instead of letters to indicate direction of air flow? There are some distinct gains from such a usage for the aerographer or chart maker of the winds. Official weather bureaus record direction on a 45-degree basis; that is, eight directions are given. It has long been felt that such records were not sufficiently detailed. Precision, detail and convenience are gained by the use of the degree.

There is no mechanical difficulty in getting continuous records of wind direction for the entire circle. Many forms of anemoscope give such records. Figure 6 gives such a record sheet based on one used at Blue Hill for 35 years. The eight cardinal directions are noted; but instead of 32 points of the compass, as heretofore, the intervals are at 10 degrees, and thus 36 divisional lines appear instead of the old compass point 11.25. For convenience in computation there is also introduced Greenwich Mean Civil Time, beginning at an hour appropriate for changing records on this coast, noon 75th meridian time being 17 hours Greenwich time. The sheet, however, is adaptable to any station meridian time, by inserting the proper hour in the S.M.T. column. If, however, the centesimal system is to be used, the number of divisional lines is increased to 40, and since there are 400 grads in the circle, each division represents 10 grads or 9 degrees.

[1] H. A., 83–4, pp. 169–180.
[2] *Nature*, Nov. 4, 1919.

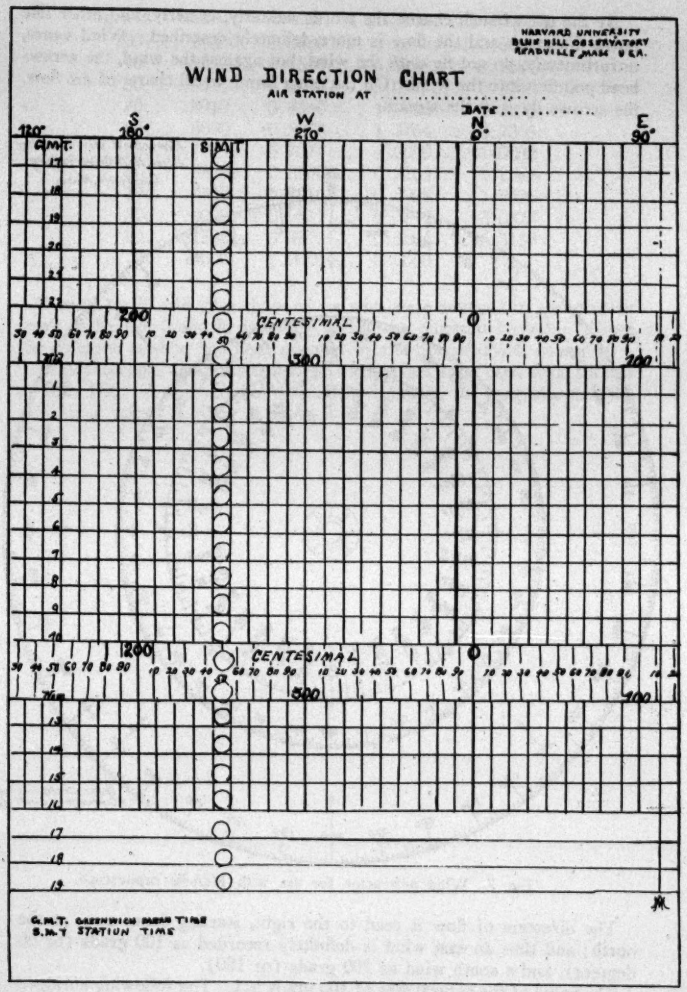

FIG. 6. Wind-direction chart.

By the use of such charts, the words westerly, easterly, and other like terms disappear and the flow is more definitely described. Wind vanes, unfortunately, do not fly *with* the wind, but against the wind, the arrowhead pointing into the wind. On the other hand, in all charts of air flow, the arrows fly with the stream.

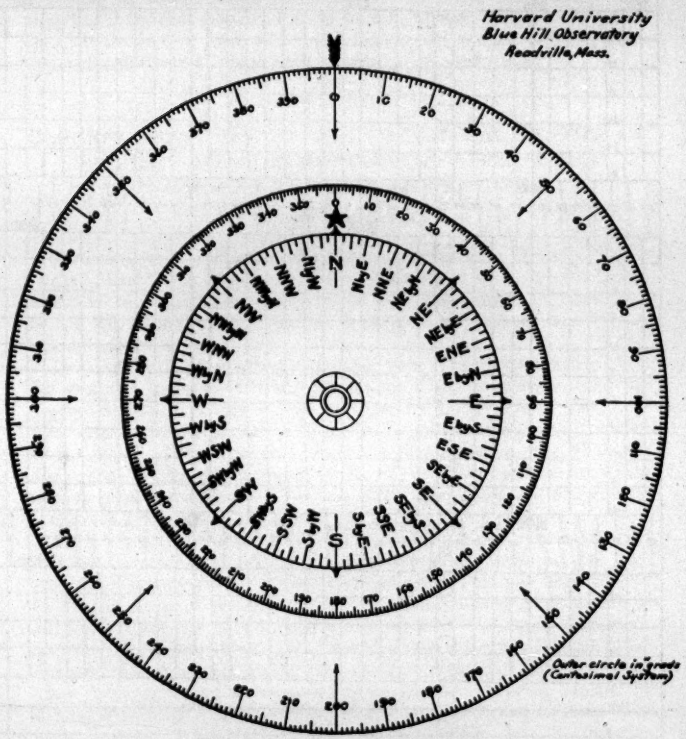

Fig. 7. Wind protractor for use with McAdie nephoscope.

The direction of flow is read to the right, starting from zero, at the north; and thus an east wind is definitely recorded as 100 grads (or 90 degrees), and a south wind as 200 grads (or 180).

The value of the natural sine of 100 grads is 1. The following abridged table gives the sines, cosines, tangents and cotangents of every 10 grads:

grads	sine	cosine	tangent	cotangent
100	1.00000	0.0000	∞	0.0000
90	.9877	0.1546	6.3138	0.1584
80	.9511	0.3090	3.0777	0.3249
70	.8910	0.4540	1.9626	0.5095
60	.8090	0.5878	1.3764	0.7265
50	.7071	0.7071	1.0000	1.0000
40	.5878	0.8090	0.7265	1.3764
30	.4540	0.8910	0.5095	1.9626
20	.3090	0.9511	0.3249	3.0777
10	.1546	0.9877	0.1584	6.3138
0	.0000	1.0000	0.0000	∞

During the World War those of us who were engaged in aerographic work in France found it necessary to use the centesimal system. Since the war, the method has been adopted by the Scandinavian countries.

In nephoscopic determinations, the method has been used with success at Blue Hill. A comparative dial of the compass, the magnetic and the centesimal values is given in figure 7.

A SINE GALVANOMETER FOR DETERMINING IN ABSOLUTE MEASURE THE HORIZONTAL INTENSITY OF THE EARTH'S MAGNETIC FIELD[1]

By S. J. Barnett

A brief historical statement was made with reference to the measurement of the horizontal intensity of the earth's magnetic field by electrical methods, and a general description of sine and tangent galvanometers was given, with the suggestion of an improvement in the latter. Then followed a detailed description of a new sine galvanometer, constructed, with certain exceptions mentioned below, in the workshop of the Department of Terrestrial Magnetism.

The base of the instrument, including the tripod, circles, etc., was taken from one of Wild's theodolites, constructed by Edelmann, and was much improved by the substitution of non-magnetic parts for parts too magnetic, and by the substitution of electrical illumination of the precision circle for daylight illumination by mirrors.

The magnetometer-box is of pure copper, the damping being chiefly electro-magnetic. The magnet-mirror is a fine disc of chrome steel with optically flat and parallel surfaces, being in fact one of the gages made by the Bureau of Standards. The torsion tube and head are similar to those of the C. I. W. magnetometers. A suspension of phosphor-bronze strip with torsional constant about 0.001 is generally used. The telescope is small but powerful; the scale is ruled to thirds of mm., on white pyralin, with all necessary adjustments. The period of the magnet and the damping, which is adjustable, are such that readings require only a few seconds.

The arrangement of coils is approximately that due to Helmholtz. The spool was machined from white Carrara marble impregnated with paraffin at a temperature near its boiling point. The coils were wound under tension in a single layer in spiral grooves cut with a carbon diamond tool. The wire is pure copper, especially prepared in the research laboratory of the General Electric Co. Each coil is wound in two halves and contains 10 turns with a diameter of approximately 30 cm. and a pitch of approximately 2 mm. The two halves start from the same horizontal plane 180 degrees apart, so that the distance between centers of adjacent wires is approximately 1 mm. The axial distance between the centers of the two coils, or the distance between corresponding turns of the spirals, is approximately 15 cm. The insulation resistance between adjacent wires is very high.

The methods of measuring the diameters and axial distances of the spirals were briefly described and some of the results were given in tables

[1] Abstract of the report presented before the American Geophysical Union, Washington, D. C., April 18, 1921.

and curves, projected on the screen. The magnetic tests, of three kinds, proving the materials to be satisfactory, were also described.

The theory of the instrument, the method of using it, and the calculation of the error in the constant of the coils due to construction, as well as of the other errors introduced in the measurement of the horizontal intensity, were briefly presented.

It was shown that the errors in reading the circle and the telescope scale when sufficiently large angles are used, and the error in the constant of the coil, were quite negligible; and that the only other error necessary to consider, viz, that introduced in the measurement of the current traversing the coils, can also be made entirely negligible. In consequence, the horizontal intensity of the earth's magnetic field can be determined with an error less than 1 part in 10,000, which more than fulfills all necessary requirements.

The instrumental work, done in the shop of the Department, chiefly by Mr. G. H. Jung, instrument-maker, is highly satisfactory.

The report was closed with acknowledgments.[1]

Department of Terrestrial Magnetism,
Carnegie Institution of Washington.

ACTIVITY OF THE EARTH'S MAGNETISM IN 1915

By D. L. Hazard

At the meeting of the International Commission for Terrestrial Magnetism held at Innsbruck in 1905 a resolution was adopted recommending that magnetic observatories classify each day according to its magnetic character as quiet, moderately disturbed, or severely disturbed, using the notation 0, 1, and 2 for this purpose. This recommendation has been adopted by different observatories, one after another, so that now nearly all of the prominent observatories are sending quarterly reports of the magnetic character of days to the Netherlands Meteorological Institute and that institution is publishing them, thus making the data available for all. While this method of characterization is necessarily rough and influenced by the personal equation of the observer, yet the mean of a large number of estimations (between 35 and 40 at the present time) gives a very good idea of the relative magnetic condition of the whole earth from day to day. It does not, however, give an absolute measure of the daily fluctuations of the earth's magnetism nor does it permit a comparison of conditions in different parts of the earth.

In order to determine quantitatively as well as qualitatively the varia-

[1] Since the presentation of this report, the constant of the coil has been redetermined by the use of many additional linear measurements, and two series of simultaneous determinations of the horizontal intensity with the sine galvanometer and the C. I. W. standard magnetometer No. 3 have been made, Messrs. Fleming, Fisk, Peters, Ives, and Barnett participating. The results obtained showed a satisfactory agreement between the two different types of instrument. A complete account of the instrument is given in Vol. IV of the "Researches of the Department of Terrestrial Magnetism."

bility of the earth's magnetism as a whole, the late Doctor Bidlingmaier devised a method which takes as a measure of the activity of the earth's magnetism its departure from moment to moment from its normal or undisturbed condition. As we have no means of determining as yet what the normal magnetic condition of the earth is, he adopted as the basis for his computations the mean value for the period under discussion; that is, the activity for a day is based on the momentary departures from the mean value for the day. He found that in determining the activity for the day with reference to the mean, the computation could be separated into two parts, first the departures of the hourly mean from the mean for the day, and second the departures of the individual values in each hour from the mean value for that hour. If it later should become desirable to refer to a base value other than the daily mean it would only be necessary to add a third term, depending on the difference between the daily means and the new base value. In each step the mean of the squares of the departures from the base value is computed and this quantity expressed in γ^2 must be divided by 8π to get the activity expressed in terms of the unit 10^{-10} erg/cm^3; that is, the energy per unit volume.

The regular observatory tabulations contain the data for computing the first part provided the mean ordinate for each hour is tabulated, as is now the established practice. Computation of the second part would ordinarily involve the reading of ordinates at frequent intervals for each hour and the computation of the mean of the squares of the differences from the mean value for the hour, this being the so-called hour-integral used in Bidlingmaier's formula in determining that portion of the activity. The amount of work involved would, of course, be prohibitive and he accordingly simplified the process by reading the ordinates for a limited number of hours and using the relation between the hour-integral and the hourly range as a basis for determining the hour-integral for the remaining hours from the hourly range. When the results were plotted with amplitude (half range) as abscissa and hour-integral as ordinate, it was found that the line joining the plotted points formed a smooth curve of parabolic form. While it was found that the values of hour-integral corresponding to a given hourly range differed considerably among themselves, as would naturally be the case because of the varying character of the fluctuation within the period of an hour, yet it was believed that for most purposes, where the results would be combined to obtain mean values, the relation between hourly range and hour-integral derived from a limited number of hours could safely be used in determining the hour-integral for a long period, as for a year.

The parabolic form of the curve representing the relation between amplitude and hour-integral suggested the probability that a linear relation would be found to exist between the square of the amplitude (or range) and the hour-integral. In fact, this must necessarily be the case if the value of activity is to be independent of the sensitivity of the instruments.

At the request of the International Commission for Terrestrial Mag-

netism a number of observatories undertook to compute the activity of the earth's magnetism according to this method for each day of the year 1915, the Coast and Geodetic Survey carrying out the work for its magnetic observatory at Cheltenham, Maryland. It was suggested by the International Commission that other observatories might safely accept the relation between hourly range and hour-integral as determined by Bidlingmaier for Wilhelmshaven for the year 1911. It was thought best, however, by several observatories, to re-determine this relation in order to be assured that it did not change from place to place. The results show that while for Wilhelmshaven the hour-integral was equal to 11¼ percent of the square of the range, for Cheltenham the factor was 10 percent and for Seddin, near Potsdam, 8¾ percent, and an investigation by Chree, which included the study of the records of the British Antarctic Expedition, showed that while for ordinary latitudes the variation in the factor was not great, conditions were quite different in very high magnetic latitudes.

When the preparation of this paper was undertaken it was expected that it would be possible to compare the results from several observatories, but it was found that only the Seddin results, in addition to those for Cheltenham, were available in printed form.

The geographic positions and mean values of the magnetic elements for 1915 for these two stations are as follows:

Observatory	Cheltenham	Seddin
Latitude	38° 44′ N	52° 23′ N
Longitude	76 50 W	13 04 E
Declination	6 04 W	8 17 W
Dip	70 47 N	66 25 N
Horizontal intensity	19417γ	18726γ
Vertical intensity	55694γ	42898γ
Total intensity	58982γ	46808γ

It will be seen that the two observatories differ very nearly 90° in longitude, and while Seddin is much farther north than Cheltenham, the magnetic dip and intensity are much greater for Cheltenham, the latter station being nearer the magnetic pole.

The activity has been computed for each hour for D, H, and Z at Cheltenham and for X, Y, and Z at Seddin, and these three are combined to get the total activity. The quantities published are the mean value for each day of the year and the hourly means for each month.

In discussing the results of this method of determining the activity three things must be kept in mind. First, since the activity is based on the *square* of the departure from the mean value, a day of very large disturbance will have an overpowering effect on mean values in which it enters. For example, the activity at Cheltenham for June 17 was 1477 and the total for the other 29 days was only 666. Second, Bidlingmaier's conception of activity is different from the usual idea of activity as represented

by a magnetic disturbance. In the latter case we think only of abnormal variations, whereas he includes all variations, whether systematic (as diurnal variation) or abnormal. Third, as part of the variation of the earth's magnetism is a function of local mean time and part is a function of absolute time, the results for different observatories are not strictly homogeneous. For this reason a more satisfactory agreement between the results for different places may be expected if only that part of the activity is considered which is derived from the hour-integral, as this is to a greater extent independent of local mean time. Even then, however, there is some lack of homogeneity, as for example, in the case of the Seddin tabulations the day begins at Greenwich midnight, whereas for Cheltenham it begins at 5^h G.M.T.

The portion of the activity derived from the hour-integral is much smaller than the part depending on the differences between the mean hourly values and the daily mean, only about one-eighth as great on the average for all days and only one-twenty-fifth for the less disturbed days. The fact that the normal diurnal-variation is such a predominant factor in the total activity as derived by Bidlingmaier's method raises the question whether results of greater value would not be obtained if the diurnal variation was eliminated, at least in part, in computing the activity.

A comparison of the daily mean values of total activity for the two observatories for 1915 shows a general agreement, but with considerable difference in detail, largely because of the third point referred to above. The total activity is on the average about 15 percent greater for Cheltenham than for Seddin, as was to be expected on account of its higher magnetic latitude, but for many days and even for some monthly means the Seddin values are greater.

If only the hour-integral activity is used in the comparison, the agreement is much closer, and when the results are smoothed out by taking five-day means the plotted curves for the two places are almost identical in phase. The agreement is almost as good with the international character numbers, both in phase and relative amplitude, and speaks well for that simple method of determining the degree of disturbance.

A comparison of the monthly means with the relative sun-spot numbers for the same periods shows little evidence of systematic agreement, thus, as pointed out by Schmidt,[1] confirming former experience in comparing these numbers with terrestrial phenomena. In this case the lack of agreement is no doubt partly due to the exaggerated effect on the mean activity for a month of a single day of great disturbance.

As to the diurnal variation of the total activity, the predominant feature is a maximum occurring about noon local mean time. At Cheltenham this feature is modified in the months May to August to form a two-peaked summit with maxima about 10^h and 14^h and a considerable depression between. There is little variation in activity during the night hours. The range of activity is greater in summer than in winter, though, as

[1] *Terrestrial Magnetism*, Sept., 1920.

already pointed out, the effect of a single day of great disturbance is overpowering. The above characteristics can be traced at once to the features of the diurnal variation of the magnetic elements, the two maxima at Cheltenham corresponding to the minimum horizontal intensity before noon and the maximum west declination after noon.

If we consider only the hour-integral activity, we find very little evidence of system in its diurnal variation in the different months, but there seems to be a tendency toward higher values toward the end of the 24 hours. This is more pronounced for Seddin and corresponds to the distribution of disturbed hours arrived at directly. The effect of a few disturbed hours is so great, however, that it is hardly safe to draw definite conclusions from the results of a single year.

Schmidt discusses some other phases of the activity at Seddin in 1915, in the paper referred to above, but time does not permit going into the matter in greater detail here. He also makes some comparisons with the results of simpler methods of determining the activity.

As a result of the activity computations for 1915, I am of the opinion, which is shared by Schmidt and Chree, that equally valuable results can be obtained by other methods that involve much less time and labor than Bidlingmaier's. His method gives undue weight to days of large disturbance in any combination of hourly values, and the introduction of the term depending principally on the diurnal variation of the earth's magnetism prevents a satisfactory comparison of results at different stations.

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ON MEASURES OF THE EARTH'S MAGNETIC AND ELECTRIC ACTIVITY AND CORRELATIONS WITH SOLAR ACTIVITY

By Louis A. Bauer

When attempting to find correlations between manifestations of the sun's activity and those of the earth's magnetic and electric activity, three points require immediate consideration:

(1) What shall be taken as an adequate measure of the sun's activity with respect to such radiations and emanations as are likely to have an effect upon the magnetic and electric fields of the earth?

(2) What shall be taken as an adequate measure of the earth's magnetic activity, or of the earth's electric activity?

(3) What quantities shall be taken as defining the so-called normal or undisturbed condition of the earth's magnetic field, or of the earth's electric field?

With respect to the first question, we have at present at our disposal the sun-spot numbers, sun-spot areas, flocculi areas, prominences, faculæ, and solar-constant values.

For measures of the earth's magnetic activity, as well as of its electric

activity, we may use fluctuations in the magnetic and electric quantities, which are more or less periodic in their character, as, for example, the diurnal range or annual range of the magnetic and electric elements. But it is also found that during a magnetic storm and for some time afterwards, the earth's permanent magnetic state, as also possibly its electric state, has been affected. Thus, we have at our disposal both fluctuations about a mean value for a certain interval, and also change in that mean value for a given time. The selection of normal or undisturbed values of any measure taken may be based upon the international list of so-called magnetically-calm or electrically-calm days. Though it must not be overlooked that often the values of the magnetic and electric elements on such days are affected by a peculiar kind of disturbance. In brief it has been found that the magnetic or electric elements on a comparatively undisturbed day are not necessarily normal values. Rather may the values be "normal" which lie intermediate between those for the "quiet" days and those for the days of moderate disturbance.

Every analysis thus far undertaken of any particular magnetic fluctuation indicates that the observed effects are to be ascribed to at least two systems of forces: *E*, an external system consisting most probably of electric currents in the upper regions of the atmosphere; and *I*, an internal system consisting of electric and magnetic systems below the earth's surface. The two systems *E* and *I* are not necessarily related as though *I* were the result of an inductive effect caused by the system *E*. The system *I* would appear rather as a composite system, composed primarily of a direct effect and secondly of an indirect effect which may be related to the fluctuating *E* system. Indications have also been found of the presence of a third system, *C*, consisting of vertical electric currents which apparently pass through the earth's surface, either from the atmosphere or from some internal source. What we observe during a magnetic storm is the combined effect of the three systems, *E*, *I*, and *C*, and this important fact must be borne in mind in endeavoring to find correlations between solar activity and terrestrial activity. It may even happen, as apparently was the case on May 8, 1902, during the eruption of Mont Pelée, that we have a world-wide magnetic fluctuation of internal rather than external origin. Hence, were it feasible, a mathematical analysis should be undertaken first of a magnetic disturbance in order that the effects coming from external sources may be separated from those to be related to internal ones.

The question has also been raised, since at times a magnetic disturbance on the earth apparently precedes some striking manifestation of solar activity, whether there may not be also the possibility of a universe disturbance-system affecting both solar activity and planetary magnetic activity.

As the combined result of my investigations to date, it is found that, in general, the most successful measure of solar activity, of special interest here, is a quantity indicative of the variability of sun-spottedness during a

given period. For example, instead of taking the sun-spot numbers (N) direct for comparison with magnetic or electric fluctuations, the range (R) in the sun-spot numbers per month, or the average departure (D) of the daily sun-spot numbers from the monthly mean, irrespective of sign, is taken. The R and D quantities are found to run closely parallel to one another; preference was finally given to the D-measure of solar activity as it utilizes all the sun-spot numbers (N) during a month, whereas, the R-measure depends only on two numbers—the maximum and minimum sun-spot numbers of the month. The annual mean values of R and D are furthermore found to run closely parallel with the N-numbers; the monthly values of R and D, however, generally follow a decidedly different course from the N-numbers and exhibit a closer relationship with the measures of the earth's magnetic, or its electric, activity than do the latter (the N's). Some of these relationships between solar activity, terrestrial magnetism, and terrestrial electricity (earth-currents, atmospheric electricity, and polar lights) are shown in figures 1 and 2 and are summarized below.

The adopted measure of the earth's magnetic activity is a quantity, $w = \epsilon Hv$, where H is the horizontal intensity of the earth's magnetic field at the observing station and v, the observed magnetic variation, or range of the magnetic fluctuation; ϵ is a numerical factor. It may be shown theoretically that this value of w, as a first approximation, is representative of the energy-change which the earth's magnetic field experiences during a magnetic variation.

Under certain assumptions it may also be shown that the R and the D measures of solar activity may be regarded, as a first approximation, as representing an energy-change experienced by the sun during a manifestation of activity.

Obtaining similarly, as just described for the sun-spot numbers, R and D measures of solar activity from the solar-constant values (E), which have been observed under the auspices of the Smithsonian Institution at Calama, Chile, during 1919 and 1920, it is found that these latter measures run much more closely parallel with the R and D measures derived from sun-spottedness than do the numbers E and N.

Connections between sun-spot activity, disturbances of the earth's magnetism, earth-currents, and polar lights have been worked out by various investigators. The present investigation shows that there is a fifth natural phenomenon—atmospheric electricity—by which an interesting and suggestive relationship with solar activity is exhibited. Owing to the many disturbances to which the atmospheric-electric elements are subject, as for example during cloudy and rainy weather, it has been more difficult to establish the existence of definite variations of the chief atmospheric-electric elements during the well-known sun-spot cycle of somewhat over 11 years than in the case of magnetic effects, earth-currents, and polar lights. The new results found are based upon atmospheric-electric data obtained chiefly at four European observatories between 1898 and 1919,

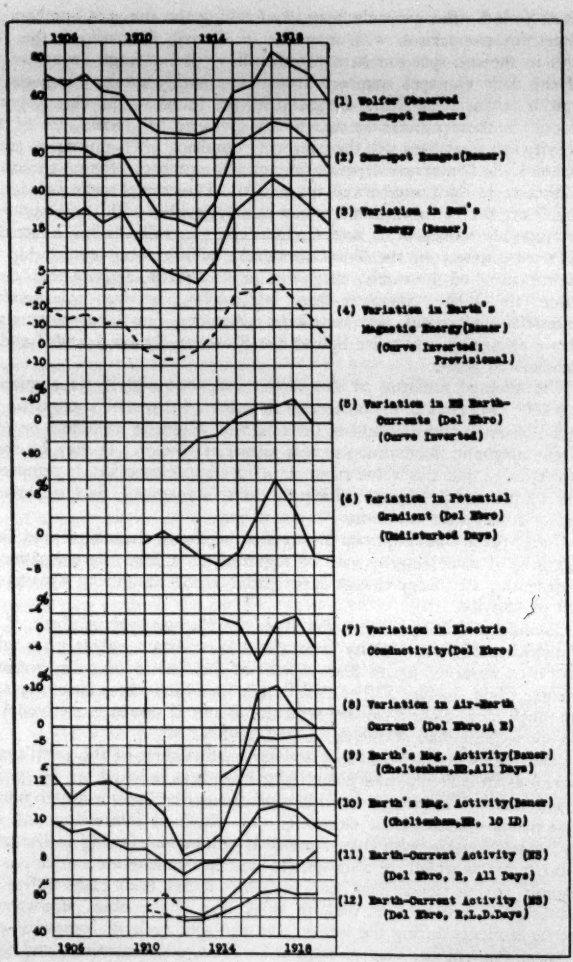
1906
1910
1914
1918
(1) Wolfer Observed Sun-spot Numbers
(2) Sun-spot Ranges (Bemmer)
(3) Variation in Sun's Energy (Bemmer)
(4) Variation in Earth's Magnetic Energy (Bemmer) (Curve Inverted; Provisional)
(5) Variation in NS Earth-Currents (Del Ebro) (Curve Inverted)
(6) Variation in Potential Gradient (Del Ebro) (Undisturbed Days)
(7) Variation in Electric Conductivity (Del Ebro)
(8) Variation in Air-Earth Current (Del Ebro; μ E)
(9) Earth's Mag. Activity (Bemmer) (Cheltenham, ER, All Days)
(10) Earth's Mag. Activity (Bemmer) (Cheltenham, ER, 10 LD)
(11) Earth-Current Activity (ES) (Del Ebro, E, All Days)
(12) Earth-Current Activity (ES) (Del Ebro, E, L.D. Days)

FIG. 1. Variations in solar activity, terrestrial magnetism, atmospheric electricity, and earth-currents during 1905–1920.

the combined data in the case of the potential-gradient thus covering about two sun-spot cycles. Recent observations on board the *Carnegie* also indicate a decrease in the electric potential-gradient since 1917, when sun-spot activity was at a maximum. A half century ago Quetelet at Brussels and

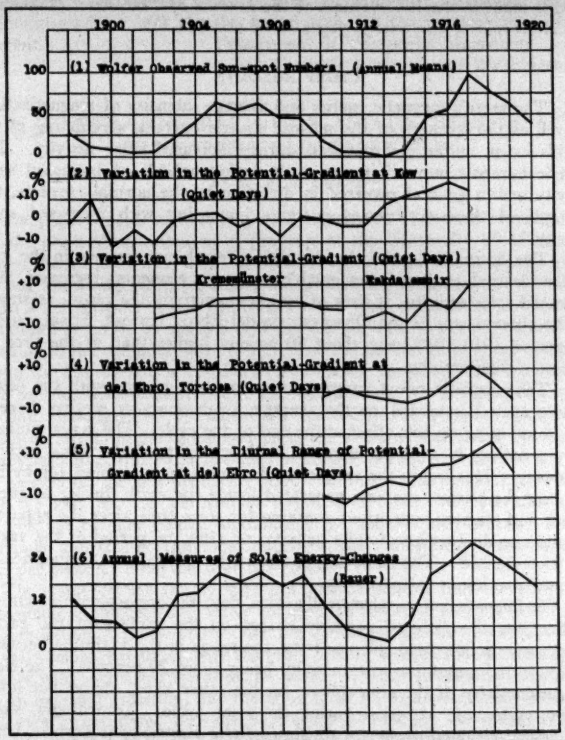

FIG. 2. Variation of the electric-potential gradient and of its diurnal range during sun-spot cycle. (See also Fig. 1.)

Wisclizenus at St. Louis believed that they obtained some definite results showing a variation in the potential-gradient dependent upon sun-spottedness. Owing, however, to the uncertainty of results obtained by the instrumental methods then in use and because of the necessity of thoroughly eliminating the numerous disturbances dependent upon meteorological

conditions, these previous results have not been accepted, and so modern treatises on atmospheric electricity omit mention of any possible relationship between atmospheric electricity and solar activity.

More complete publication of the results of the investigations here outlined is made in the 1921 volume of the *Journal of Terrestrial Magnetism and Atmospheric Electricity*, pages 33-68 and 113-115.

CHIEF RESULTS

1. The earth's magnetic energy and average intensity of magnetization, as well as the strength of the normal electric currents circulating in the earth's crust, suffer a diminution during increased solar activity. The electric currents induced in the earth during periods of increased solar activity are in general reversed in direction to the normal currents, the strength of these superposed currents increasing with increased solar activity.

2. The diurnal range of the strength of earth-currents, as in the case of the diurnal range of the earth's magnetic elements, increases with increased solar activity; at time of maximum activity the range, as shown by the observations at the Observatorio del Ebro, Tortosa, Spain, during the period 1910–1919, was about 50 percent higher than at the time of minimum solar activity.

3. The magnetic effect running a concomitant course with the solar-activity cycle is retarded, on the average, about one year so that there is a residual, or an acyclic, effect at the end of the cycle. The actual amount of retardation, in general, increases with intensity of the sun's activity or energy. This lag in the magnetic effect may be accounted for by the fact that the electric currents generated inside the earth during magnetic storms and magnetic variations continue for some time after the apparent cessation or diminution of solar activity, or after the period of the variation experienced. The same lag is shown by polar-light frequencies at times of maximum solar activity.

4. On fine-weather, or electrically-calm, days the atmospheric potential-gradient, or the deduced negative charge on the surface of the earth, increases with increased solar activity, the range in the variation between minimum and maximum solar activity being about 20 percent. The electric conductivity of the atmosphere, on the other hand, shows but little, if any, systematic variation during the solar cycle. Accordingly, since the vertical conduction-current of atmospheric electricity is derived from the product of the potential-gradient and the electric conductivity, it is found that this vertical current also increases in strength with increased solar activity. It would thus appear that atmospheric electricity, like terrestrial magnetism, is controlled by cosmic factors. The results derived here may have an important bearing upon theories of atmospheric electricity.

5. The diurnal range of the electric potential-gradient as deduced from the observations on the electrically-calm days, made at the Observatorio

del Ebro, Tortosa, Spain, 1910–1919, is found to increase with solar activity; the minimum occurred in 1911 and the maximum in 1917, whereas the sun-spot minimum occurred in 1912 and the maximum in 1917. The range between minimum and maximum diurnal range is about 25 percent. (It appears probable that the same fact just stated for the potential-gradient will also be found true for the vertical conduction-current.)

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THE PENETRATING RADIATION AND ITS BEARING UPON THE EARTH'S ELECTRIC FIELD[1]

By W. F. G. Swann

The paper was devoted largely to a description of certain investigations on the penetrating radiation in progress at the University of Minnesota, under the author's direction. It opened with a brief review of the status of our knowledge with regard to the penetrating radiation.

In a hermetically sealed zinc vessel freed from radioactive air, ions are produced at a rate of about 8 or 9 per c.c. per second over the land. According to the computations of A. S. Eve, the normal gamma-ray radiation from the atmosphere is capable of accounting for about 0.06 ion per c.c. per second, while that from the soil will account for 1.6 ion per c.c. per second, making in all 1.7 ions. On account of the secondary ionization resulting from electrons emitted from the walls of the vessel by the primary radiation, this value becomes increased to about 2.5 ions in the case of vessels of the size ordinarily used. If this value be subtracted from the 8.5 ions per c.c. per second found over land, there remains about 6 ions per c.c. per second attributable to causes which are not directly obvious, and this corresponds roughly to the ionization (4 to 6 ions per c.c. per second) observed over the ocean.

The results of several investigators seem to indicate that, in vessels surrounded by thicknesses of water sufficient to absorb practically all gamma-ray radiation of the ordinary type, the ionization does not become reduced greatly below that found over the great oceans, nor does it diminish appreciably with increasing thickness of water, so that, if this ionization is due to an external radiation, this radiation must be of an extremely penetrating type as compared with ordinary gamma-radiation. By using a vessel of ice, in order to be as free as possible from radioactive contamination, McLennan obtained a value as low as 2.6 ions per c.c. per second over Lake Ontario, and inclines to the view that even this small residual is attributable to lack of complete purity of the ice. On the other hand, results obtained by Kolhörster in balloon ascents up to 9.5 kilometers show an increase to about 80 ions per c.c. per second at this altitude, in a her-

[1] Abstract of the paper presented at the annual meeting of the American Geophysical Union, Washington, D. C., April 18, 1921.

metically sealed, light, tight vessel; moreover, the rate at which the apparent ionization increases with altitude in the neighborhood of 9 kilometers is such as to suggest that at altitudes but slightly greater, the ionization might attain enormous values. The validity of Kolhörster's results has been questioned by C. H. Kunsman in view of possible complications resulting from the effect of the low temperature on the insulating material used; if, however, they should be substantiated, they afford one of the most convincing evidences that one could wish as to the true cosmic nature of part at least of the so-called penetrating radiation. By the employment of precautions such as to prevent all uncertainty as regards leakage, and by the use of small pilot balloons, the writer is at present endeavoring to extend the observations of Kolhörster to greater altitudes.

VARIATION OF RESIDUAL IONIZATION WITH PRESSURE

The study of the variation of the residual ionization with pressure has a very important bearing upon the origin of that ionization. If the residual ionization were due to alpha-rays emitted from the walls of the vessel, it would show practically no increase with pressure in the case of a vessel whose linear dimensions were of an order of magnitude greater than the range of the alpha-particles. Analagous remarks hold for the case of soft beta-rays emitted from the walls. In the case of a radiation of cosmical origin there are three possibilities, viz, (1) a direct ionization by the primary radiation, (2) ionization by slowly moving electrons emitted from the gas by the primary radiation, (3) ionization by rapidly moving electrons emitted from the gas by the primary radiation, the penetrating power of the electrons being such as to enable them to go right across the vessel at atmospheric pressure. A fourth possibility resulting from emission of electrons from the walls of the vessel by the primary rays, is indistinguishable from a corresponding emission resulting from radioactive contamination. Ionization of the first and second type would increase proportionally with the pressure until the pressure attained was so high that the primary radiation itself became appreciably absorbed in passing through the vessel. Effects resulting from the third type of ionization require more detailed consideration.

At each point in the gas there will be a definite value of what we shall call the ionizing intensity I, i.e., the number of ions which would be produced per c.c. per second in an element of gas at *atmospheric pressure* placed at the point in question. For the purpose of this definition, we may suppose the element of gas to be contained in a non-absorbing capsule, since it is of course not implied that the whole of the gas is at atmospheric pressure. The number of ions produced per c.c. per second in the gas at some point O where the pressure is p will be Ip.

Let us consider an element of volume $ds\ dr$, situated at a point P at a distance r from O in some definite direction, dr being element of radius vector, and ds being element of cross section of an elementary cone of

solid angle $d\omega$ drawn from O to P. The contribution of the element of volume to the value of I at O is

$$\Delta I = \frac{pN}{r^2} f(pr)\, ds\, dr$$

where, assuming the secondary radiation originating within an element of volume to be proportional to the pressure therein, $pN\, ds\, dr$ is the secondary ionization intensity at unit distance from P in the direction PO, on the basis of no absorption, and $f(pr)$ is a factor inserted to take account of the absorption in passing from P to O, and also of the variation of the ionizing efficiency along the path of the ionizing agent. By writing $f(pr)$ instead of $f(r)$, we imply that the diminution of the ionizing intensity per unit distance (apart from spreading) is proportional to the number of molecules per c.c. In terms of the solid angle $d\omega$ above referred to we have:

$$\Delta I = N f(pr)\, p\, dr\, d\omega$$

If we should now increase the pressure from p to p_1, and diminish r to r_1 so that $pr = p_1 r_1$, and dr to dr_1 so that $p dr = p_1 dr_1$, the new element of volume contained between the radii r_1 and $(r_1 + dr_1)$ in the cone $d\omega$ will make the same contribution to the ionizing intensity at O as did the old element of volume comprised between r and $(r + dr)$. The sum total of all the elements of volume in the vessel, corresponding to the lower pressure will, however, correspond to a sum total of elements of volume which, at the higher pressure, do not fill the vessel. Hence, the actual ionizing intensity at O will be greater at the higher pressure than at the lower pressure. Since q, the number of ions produced per c.c. per second at O, is obtained by multiplying I by the pressure p in atmospheres, we see that the increase in q, per atmosphere increase, should itself increase with the pressure. We may extend this statement so as to include in q the ionization due to the direct action of the external radiation, since this increases proportionally with the pressure. Thus, the actual ionization in the vessel, due to primary and secondary emission from the gas, will be less at one atmosphere than at any higher pressure. If then, the ionization-pressure curve should show a very small increase of ionization per atmosphere increase at high pressures, we know from the above that such increase per atmosphere is nevertheless greater [1] than the portion of the ionization due to primary and secondary action in the gas within the vessel at one atmosphere. We may infer that any greater ionization found at atmospheric pressure is to be attributed to radiation from the walls of the vessel; this radiation, owing to its absorption at the higher pressures, results in a diminishing rate of increase of ionization with pres-

[1] It would not be quite safe to extend this argument to imply that the ionization here referred to was necessarily greater than the true natural ionization in the open air, since a portion of the ionization in a volume of the external air occupying the space of the vessel would result from secondary radiations originating outside of that volume.

sure. The foregoing discussion has been made for the case where the primary radiation is so penetrating as to be but little absorbed in passing through the gas in the vessel, even at the higher pressures, this being the case which is of interest in discussing the action of a radiation whose degree of penetration is comparable with that attributed to the cosmical penetrating-radiation.

Experiments on the variation of the residual ionization with pressure have been made by several investigators, the most recent being those made under the writer's direction, by Dr. K. M. Downey and by Mr. H. Fruth. The main feature of the experiments of the latter investigators lay in the use of a comparatively large vessel (a sphere one foot in diameter), and the employment of certain special devices to insure freedom from errors due to leakage and lack of constancy of the batteries. Dr. Downey's observations extended in the first instance up to 20 atmospheres, giving over this range a practically perfect linear variation with the pressure and an increase of ionization of about 1.2 ions per c.c. per atmosphere increase. On extending the observations to higher pressures, it was found that the linear relation ceased to hold in the neighborhood of about 27 atmospheres, and the curves finally became parallel to the pressure axis at pressures above 46 atmospheres. If one were to accept this parallelism without reservation, he would be forced to conclude that the portion of the ionization within the vessel which was attributable to the direct or indirect action of the radiation on the gas was immeasurably small.

Mr. Fruth's observations have been made for air, oxygen, and nitrogen up to pressures of 75 atmospheres, and for carbon-dioxide up to its liquefying pressure, with sensibly the same results for all the gases used. While his curves do not attain as complete a parallelism with the pressure axis as do those of Dr. Downey, they correspond to an increase per atmosphere of less than 0.75 ion per c.c. per second at the higher pressures.[1] It is worthy of note that the presence of radium-emanation in the gas would tend to increase the slope of the ionization-pressure curves. The normal emanation-content of the atmosphere is such as to produce about 2.3 ions per c.c. per second. Each additional atmosphere of air would, of course, carry with it its emanation-content, so that the increase per atmosphere for normal air resulting from the emanation-content alone would amount to 2.3 ions per c.c. per second per atmosphere increase. It is therefore necessary to carefully age the air before use. Recalling that the emanation activity dies to half value in 3.85 days, it will be readily seen that in the case of air 3 weeks old the effect of the emanation would become reduced to a negligible amount. In some of Dr. Downey's observations the air was aged for a month before use.

In experiments of this kind it is of the utmost importance to insure complete saturation, and this matter was consequently tested very care-

[1] Since this was written Mr. Fruth has found complete parallelism for air, oxygen, and carbon dioxide for pressures above 52 atmospheres, when the gases are perfectly dry and dust-free.

fully, the voltages used being considerably higher than those at which experiment showed saturation to have been attained at the higher pressures. One element of uncertainty not usually considered in relation to the attainment of saturation must be here referred to. It pertains to the effect of dust nuclei. Such nuclei could theoretically cause a departure from a saturation which could not be reduced beyond a certain minimum however great the field might be, since increase of the field intensity could not reduce beyond a certain limit the probability of an ion's encounter with a dust nucleus during its passage across the vessel. Departure from saturation due to such a cause would not show up by the failure to attain apparent saturation with increasing field, and it would increase, moreover, with increase of the amount of gas (and consequently of dust nuclei) in the vessel. The comparatively good agreement between the results of Dr. Downey and those of Mr. Fruth, and the agreement of the various results of the latter investigator among themselves, suggest, however, that dust did not play an important rôle in the experiments, particularly when one remembers that the various experiments corresponded to different samples of gas, samples which had undergone, moreover, entirely different treatments. However, it is planned to make a very careful investigation of the effect of dust in this connection; for, if the experiments of Dr. Downey and of Mr. Fruth represent a primary phenomenon, not explicable by subsidiary considerations of this kind, they carry with them the very remarkable conclusion that, of the ionization observed in a vessel at atmospheric pressure and ordinarily attributed to a penetrating radiation, less (and probably considerably less) than one ion per c.c. per second is to be accounted for as having its origin in a direct action of the primary radiation on the gas or in the action of a secondary radiation emitted from the gas by the primary radiation.

EXPERIMENTS ON THE DIRECTION OF THE PENETRATING RADIATION

An important light would be thrown upon the origin of the penetrating radiation if it could be shown to partake of a directive character. Experiments on this matter were originally made by Cook and by Wood, who interposed screens between the apparatus and its surroundings at various orientations. The experiments were inconclusive, but, as far as they went, seemed to indicate that the radiation came equally from all directions. A method of this kind is very seriously affected by lack of constancy of the residual ionization itself during the various experiments between which comparisons are subsequently made, and, for this reason, some experiments were undertaken by Miss J. Herrick under the writer's direction with the object of eliminating the main causes of uncertainty in the earlier experiments of Cook and Wood. The method used by Miss Herrick depends upon the fact that if gamma-rays pass through a thin sheet of metal, the ionizing electrons emitted from the side at which the rays enter differ as regards their number and speed from those which are ejected from the

side at which the gamma-radiation leaves. If the penetrating radiation is of a gamma-ray type, it should show similar characteristics. The ratio of the subsequent ionization resulting from the incidence of gamma-rays on a surface to that resulting from the emergence of gamma-rays from the surface depends upon the material of the surface. The apparatus used consisted of two similar cylinders mounted with their axes in the same horizontal line. One semi-circular half of each cylinder was made of lead, and the other half was made of aluminum. The cylinders were provided with central rods which were connected to each other and to the insulated quadrant of an electrometer. By placing potentials of plus fifty and minus fifty volts respectively on the cylinders, the ionization currents in the gas could be caused to feed into the electrometer in such a way as to almost completely compensate. Several precautions to avoid leakage, and errors due to fluctuation in the potentials of the batteries were taken, the details of which it will be unnecessary to describe.

To fix our ideas, suppose that an excess of gamma-radiation comes from above, and that in the case of one of the cylinders the lead half is uppermost. Then, as far as ionization due to the emission of electrons from the wall of the vessel is concerned, the ionization in the vessel in question (or rather the portion of it due to the excess of radiation coming from above) will be due to the emergence radiation from the lead, and the incidence radiation from the aluminum. An alteration of the effect should consequently be produced by rotating the cylinder through 180 degrees, while the other cylinder is left untouched, its function being simply to act as a compensator for the purpose of minimizing effects resulting from an actual variation in the conditions during the experiment. Without here entering into details, it will be seen that it would be possible to obtain a comparison between the radiation coming in different directions and the total radiation entering the vessel in so far as the ionization was due to the electrons emitted from the walls of the vessel and in so far as one assumed the penetrating radiation to partake of the nature of a hard gamma-radiation as regards the difference between the incidence and emergence effects in the case of the metals used.

The first experiments performed by Miss Herrick in the laboratory of the physics building showed marked changes on rotating one of the cylinders, and by plotting a polar diagram representing the ionization due to radiation coming from the various directions it became possible to locate small quantities of radium in different parts of the building. The polar diagram, moreover, showed a hump indicating an excess of radiation coming from above. The apparatus was next moved to the astronomical observatory, where there was no radioactive material, and experiments again gave an indication of an excess radiation from above. In order to be free from all possibility of radioactive contamination, the writer next set the apparatus up in the attic of his house, and carried on observations over a period of 6 weeks, the observations being taken between the same hours each day. The attic of a dwelling house is not the most ideal situa-

tion for a quadrant electrometer, but on plotting the results for the various experiments there was again decided evidence of an excess radiation in the downward direction. The magnitude of the excess was such that, when the vessel was turned in the most favorable direction, the ionization was about 9 percent greater than the average, a result in comparatively good agreement with the observations of Miss Herrick made on the university campus.

These experiments are only cited as of a preliminary nature, for there are certain sources of complication which must be removed before a cosmical interpretation may be made of the results. Thus, the potential-gradient in the atmosphere will deposit active material from the atmosphere on the roof of the building in which experiments are made. A simple calculation will show that the amount of such deposition may well be enough to seriously affect experiments of the kind described above. Similar remarks apply to the effect of radioactive material in the soil itself, and that deposited on the surface of the soil by the atmospheric potential-gradient. It is planned to continue the observations under conditions which, it is hoped, will eliminate these causes of uncertainty.

THE EARTH'S PENETRATING RADIATION AND THE ORIGIN OF THE EARTH'S CHARGE

In 1915, the writer proposed a theory of the origin of the earth's charge based on the assumption that high speed electrons were shot into the earth from the atmosphere as a result of a very slight radioactivity of the atmosphere itself, or as a result of the breaking up of the emanation normally in the atmosphere. If one assumes that an electron of sufficiently high speed can have a range as great as 5 kilometers in the atmosphere, it is only necessary to postulate the emission of one such high speed corpuscle per c.c. in the downward direction, each 100 seconds in order to account for the maintenance of the earth's charge. Or, viewed from another standpoint, since we know that about 5 pairs of ions are formed per c.c. per second in the atmosphere, it is only necessary to suppose that in the case of one out of every 500 pairs of ions formed a high speed corpuscle of the above kind is emitted. The theory of passage of electrons through matter is not at all inconsistent with the postulation of great ranges such as those required by the theory; however, in 1917 Swann [1] put forward another theory in which the expulsion of the electrons from the atoms of air is brought about by the penetrating radiation from above, the hard nature of this radiation resulting in its emitting electrons from the air molecules almost exclusively in the downward direction. Under the influence of the electronic bombardment, the earth would charge up until the conduction current back to the atmosphere just sufficed to balance the effect. It appeared that if one assumed as many as 3 corpuscles to be emitted per c.c. per second from the atmosphere, it would only be necessary to postulate a range of penetration of about 9 meters in order to

[1] *Phys. Rev.*, 9, 555-557, 1917.

account for the maintenance of the earth's charge. As pointed out by the writer at the time, the chief difficulty facing a theory of this kind is the explanation of why the swiftly moving corpuscles do not produce, in the atmosphere, a much greater ionization than is observed. Difficulties of this kind assume a much less formidable aspect, however, when viewed in the light of modern views as to the properties of swiftly moving electrons. In 1918, v. Schweidler independently put forward the second of the above theories, and described an experiment carried out with the object of testing it. The aim of this experiment was the endeavor to observe a charging effect in a thick piece of metal as a result of corpuscles entering it, the piece of metal being surrounded by a shield from which it was insulated. Failure to observe any charging effect caused v. Schweidler to conclude that the replenishment of the earth's charge could not be brought about by a corpuscular radiation of the type discussed. As a matter of fact, the writer had performed an experiment somewhat similar to v. Schweidler's experiment in 1915, in connection with his earlier theory of corpuscular charging. In this experiment an earthed vessel surrounded an insulated hollow cylinder connected to an electrometer. The rate of rise of potential was noted, and a solid copper bar was then inserted in the hollow cylinder, the rate of rise being then again noted. By this device of performing two experiments in which the surfaces exposed were the same, surface effects were eliminated. As in v. Schweidler's experiment, no certain charging effect was observed; and, while this weighed against the former of the theories above referred to, it was not felt that it formed so weighty an argument against the latter, for on that theory it might result that the penetrating radiation would shoot as many electrons out of the bottom of the cylinder as it shot in at the top, except for the absorption of the penetrating radiation itself within the cylinder. In other words, it is the coefficient of absorption of the penetrating radiation in the cylinder rather than the coefficient of absorption of the corpuscles which is of importance. In a recent paper,[1] R. Seeliger discusses the origin of the earth's charge. He considers v. Schweidler's experiment as conclusive evidence against any theory which postulates a corpuscular replenishment at all parts of the earth, but raises the question as to whether a corpuscular replenishment may not take place in certain limited areas, in polar regions for example. It is to be observed that any assumption of this kind invites, in their most serious form, difficulties associated with the ionization which might be expected to result from a passage of the corpuscles through the atmosphere. For a concentration of the corpuscular current in a limited region would result in greatly increased ionization in that region, and on the assumption that a corpuscle produces 50 ions per centimeter of its path and that the coefficient of recombination of ions is 1.6×10^{-6}, it can readily be shown that, unless the area of precipitation were more than one thousandth of the area of the earth, the conductivity produced in the air in the region of precipitation would be so great that, for a potential gradient of 150 volts per meter, one would calculate

[1] *Annalen der Physik*, 63, 464-481.

for this region alone a total conduction current greater than the corpuscular current. In other words, there is a lower limit to the value which one may assume for the region of precipitation. Thus the avoidance of difficulties concerned with the failure to directly measure a corpuscular current, by relegating that current to regions where experiments have not been made, does not avoid what is perhaps one of the most serious difficulties confronting any corpuscular theory, that of reconciling the comparatively small ionization of the atmosphere with the passage through it of about 1500 high speed corpuscles per square centimeter per second.

THE CONDUCTIVITY OF THE UPPER ATMOSPHERE

The paper concluded with a reference to the importance of a knowledge of the conductivity of the upper atmosphere in relation to the origin of the earth's charge and allied phenomena, and the author described an experiment in progress at the University of Minnesota designed with the object of measuring the distance of the supposed conducting layer by measuring the time taken by wireless waves to reach that layer and return.

RECENT RESULTS DERIVED FROM THE DIURNAL-VARIATION OBSERVATIONS OF THE ATMOSPHERIC-ELECTRIC POTENTIAL-GRADIENT ON BOARD THE *CARNEGIE*[1]

By S. J. Mauchly

The Department of Terrestrial Magnetism, in accordance with its director's plans, has for many years been making not only magnetic but also atmospheric-electric observations aboard its survey vessel, the *Carnegie*. It is thus contributing the chief data for mapping both the earth's magnetic field and its electric field. Furthermore, since 1915 numerous observations have been made aboard the *Carnegie* to determine the nature and magnitude of the changes in the electric condition of the atmosphere which take place during a 24-hour cycle.

For the *potential-gradient* the general procedure in the diurnal-variation observations is to make a set of 20 observations during each of 24 consecutive hours. The observations for such a set require about 20 minutes and their mean value is referred to the mean time for the set. From deductions based on the observations made prior to April, 1916, it appeared that the diurnal variation of the potential-gradient over the oceans probably did not differ much from that which has been found at many land stations; that is, they indicated two rather pronounced maxima and two minima during a 24-hour period.[2] However, very few data were

[1] Preliminary report presented before the American Geophysical Union, with amplifications.

[2] "Researches of the Department of Terrestrial Magnetism," Vol. III, pp. 416–420, Washington (1917).

available from oceans other than the Pacific, and as pointed out in the report just cited, a large percentage was derived from series of observations which were terminated by the advent of unfavorable weather. It should also be noted in passing that Swann[1] a year later in discussing

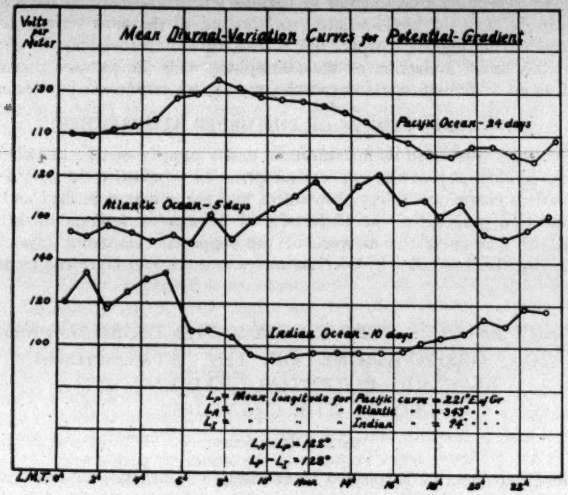

FIG. 3. Diurnal variation of electric potential-gradient on the oceans, plotted according to Local Mean Time.

the results of the observations for the year ending February 20, 1917, states that "the effect of the 12-hour Fourier wave is less important in the present curves than in those already published."

The largely increased amount of material which has accumulated since 1915 makes it now possible to reject nearly all data corresponding to less than a 24-hour series and still have 45 practically complete 24-hour series available. The data for each series, therefore, correspond to an actually occurring sequence of phenomena, and the mean results are free from the errors which would result from combining the results of partial series of observations.

Of the 45 diurnal-variation series referred to, 30 were made in the Pacific, 5 in the Atlantic, and 10 in the Indian Ocean; the combined data represent about half the earth's surface. The means corresponding to

[1] W. F. G. Swann. "Supplementary report on atmospheric-electric observations made aboard the *Carnegie* from May 17, 1916, to March 2, 1917," in "Annual Report of the Director of the Department of Terrestrial Magnetism" for the year 1917. Year Book of the Carnegie Institution of Washington, 1917, p. 282.

the separate oceans, as derived from 39 series, are represented in figure 3. They show: (1) That the mean diurnal-variation curves for the Pacific, Atlantic, and Indian oceans are similar in form; (2) that the principal component of the variation consists of a 24-hour wave, and (3) that the times of occurrence of the chief phases of this wave, when referred to

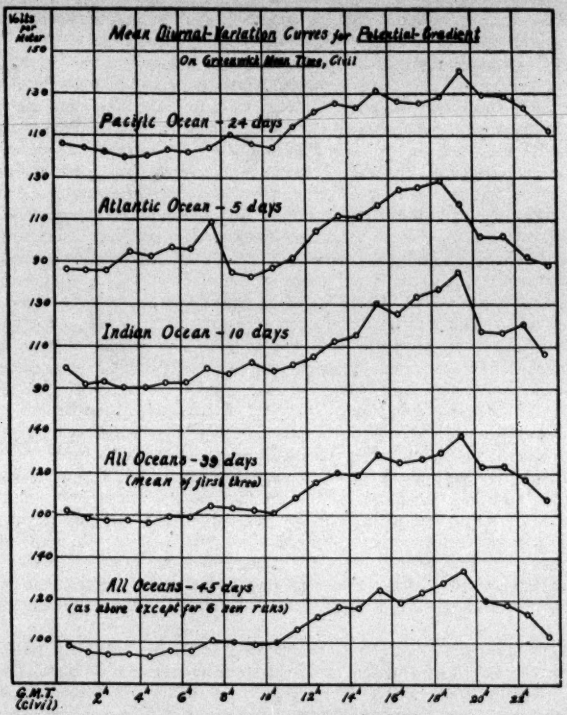
Volts per Meter
Mean Diurnal-Variation Curves for Potential-Gradient
On Greenwich Mean Time, Civil
Pacific Ocean - 24 days
Atlantic Ocean - 5 days
Indian Ocean - 10 days
All Oceans - 39 days
(mean of first three)
All Oceans - 45 days
(as above except for 6 new runs)
G.M.T. (civil)

FIG. 4. Diurnal variation of electric potential-gradient on the oceans, plotted according to Greenwich Mean Time.

local time, differ for the several oceans by amounts which correspond approximately to the differences between the respective mean longitudes, for the several oceans, of all the points at which observations were made.

Since the curves of figure 3 suggest the simultaneous occurrence of maximum (or of minimum) phase over all three oceans, it was decided

to refer the results of each series of observations to Greenwich Mean Time (civil), and recompute the means for the separate oceans on this basis. The results are shown in figure 4, together with a curve which includes the data from 6 recent series received from the vessel after the curves in figure 3 had been prepared. The differences between the several curves of figure 4 are of course not to be thought of as representative of separate characteristics, since the smoothness of the respective curves is seen to be closely related to the number of component series.

The curves of figure 4 show a decided similarity to land results for high latitudes and also to many of the winter curves obtained in ordinary latitudes. Indeed, if differences in local mean time are taken into account, it appears that for many land stations at which the single diurnal wave predominates, there is approximate simultaneity as to the time of occurrence of maximum (likewise, of minimum), and this at a time which is in general agreement with what is indicated by the curves of figure 4. For the summer, however, as is well known, most land stations show, in addition to the 24-hour wave, a decided secondary wave which seems to occur in general at about the same local mean time at different stations.

The minimum value of the potential-gradient, according to figure 4, occurs at about 4^h A.M., G.M.T., and in view of the fact that for observatories in western and central Europe the difference between local and Greenwich time is not great, this may account for the fact that various authorities have assumed the occurrence of the principal minimum at about 4^h A.M., local time, to be a rather general characteristic for most stations. It is also significant to note that Mache and v. Schweidler [1] long ago pointed out that the phase angle of the 24-hour wave varied greatly from station to station while the phase angle of the 12-hour wave was approximately the same for nearly all stations. Although the phase angles of the 24-hour Fourier waves for the European stations show among themselves very much greater differences than can be accounted for by the rather small differences in longitude, it must be borne in mind that the results of harmonic analyses are dependent upon local meteorological and cultural, and sometimes topographical and instrumental, factors as well as upon any general characteristics which the potential-gradient may possess.

In the present investigation no account has been taken of possible changes in the characteristics of the diurnal variation with latitude and with time of year, except to ascertain that the preponderance of the 24-hour wave and the approximate progress on a universal-time basis seem to hold throughout the year and for wide ranges of latitude. The present results are, therefore, to be considered as provisional and representing only a general yearly average. In fact, investigations under way show that considerable modification in detail is to be expected as more observational material becomes available. The data from 45 practically

[1] H. Mache und E. v. Schweidler, "Die Atmosphärische Elektrizität," p. 27, Braunschweig, 1909.

complete series of diurnal-variation observations aboard the *Carnegie*, representing a general distribution over most of the accessible ocean-areas indicate, therefore, as a preliminary result, that ***the chief component of the diurnal variation of the potential-gradient over the major portion of the earth (especially the oceans) is a wave of 24-hour period which occurs approximately simultaneously in all localities.***

A fact of considerable interest is that the diurnal-variation curves for the potential-gradient derived from the *Carnegie* observations are very similar to curves which represent the relative frequencies of the Aurora Borealis, as observed at several European stations, and also to curves representing the diurnal distribution of certain classes of magnetic disturbances, when all are referred to the same time-basis. It may also be pointed out that owing to the non-coincidence of the earth's magnetic axis with its axis of rotation, the time of daily potential-gradient maximum, as indicated by the ocean curves, corresponds approximately to the time when the earth's north magnetic pole, for example, is nearest to the sun, while the daily minimum occurs, in a general way, when this pole is farthest from the sun. The actual times of maximum and minimum, however, appear to depend upon the positions of *both* magnetic poles and the fact that their longitude difference is not 180°. These correlations appear to support the assumptions of various investigators that the earth's electric charge and resultant field may be very intimately related to an electric radiation from the sun. The best evidence as to the extent of this support will probably result from a study of the details of the diurnal-variation curves corresponding to various positions of the earth in its orbit. Reductions with this end in view are under way and it is hoped that sufficient data will soon be available to yield some information on this point.

The making of diurnal-variation observations in atmospheric electricity by eye readings is always a burdensome procedure; the carrying on of such work aboard a vessel is not only arduous but also difficult. In this connection the utmost credit is due the several commanders of the *Carnegie*, during her various cruises, and to all the observers who participated in the observational work.

I am indebted to the director, Dr. L. A. Bauer, for his constant interest in and encouragement of the work in hand, and for a suggestion of the possibility of finding in the asymmetry of the earth's magnetic field an explanation of the observed diurnal variation on a universal-time basis. I am also greatly under obligations to the members of the Department of Terrestrial Magnetism who assisted in the reduction of the observational data, especially to Dr. G. R. Wait, both for valuable assistance and helpful suggestions.

The full publication of the observational data and discussion of results will be deferred until after the completion of the present cruise of the *Carnegie*.

Department of Terrestrial Magnetism,
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SUGGESTIONS RELATIVE TO THE APPLICATION OF MATHEMATICAL METHODS TO CERTAIN BASIC PROBLEMS OF DYNAMIC OCEANOGRAPHY

By G. F. McEwen

Investigations of the ocean have generally been carried on by geographers and geologists, oftentimes incidentally to those of other divisions of these extensive fields of science. Accordingly, qualitative methods so characteristic of geography and geology have been widely used in oceanographic investigations. Such qualitative methods and the empirical treatment of quantitative field observations have been very suggestive, have stimulated interest, and led to certain broad generalizations that are essentially correct. However, there has been a tendency toward rather loose reasoning and lack of consideration of established quantitative principles of physics, which has resulted in certain erroneous conclusions.

Must we admit that the complexity of such geophysical phenomena renders careful reasoning and quantitative treatment impossible of attainment? Probably many would at first answer yes, but let us consider the matter further before expressing an opinion. Within the past fifty years a few scientists have undertaken, by means of a definite formulation of specific problems, to apply mathematics to ocean data, and thus to contribute to a system of demonstrable principles applicable, in general, to all similar cases; and attention has been increasingly directed to this type of research. Important advances have thus been made, and serious errors in certain former conclusions have been discovered, although, especially in some of the earlier attempts at mathematical applications, significant errors arose from incorrect assumptions and failure to appreciate important attributes of such "field," or natural problems. At first, men accustomed to the problems of laboratory physics attempted to deduce physical laws of the sea from results of laboratory studies, and certain preconceived assumptions regarding oceanic conditions. They also worked under the disadvantage of having very inadequate data. As more accurate and exhaustive field data accumulated, attention was directed more to interpreting and coordinating field observations rather than to depending on the speculative and unsound method of imposing on the sea purely theoretical laws deduced from laboratory researches.

Mohn's pioneer investigation of 1887,[1] based on the deduction of the changes in form of the surface that would give rise to currents actually produced by winds, variation in barometric pressure, and specific gravity, was a great advance beyond earlier attempts at a precise treatment of ocean data, and doubtless contributed greatly to the development of the more satisfactory methods of today. Among the later results thus worked

[1] Mohn, H. 1887. The Norwegian North Atlantic expedition, 1876–1878: The North Sea, its depths, temperature and circulation. (Christiania, Grondahl), 212 pp., 48 pls.

out are Ekman's [1] hydrodynamical theory of oceanic circulation, which pertains especially to wind-driven currents, and was undertaken at Nansen's [2] suggestion; and Bjerknes's [3] convection theory which pertains especially to the determination of ocean currents due to differences in specify gravity. Later his pupil, Sandström,[4] devised a much more rapid and accurate method of computing such currents.

The Norwegian investigator, Jacobsen,[5] in certain more recent quantitative investigations pertaining to the Atlantic near Denmark, obtained encouraging results by giving special attention to the alternating convective motion of small masses of the water, or to the "mixing phenomena," as he called it. His researches afford strong evidence in support of the idea suggested by earlier qualitative studies, that in lakes and oceans, very small or elementary masses of the water are moving at random in a manner somewhat analogous to the motion of molecules in a gas, except that the direction of motion in large bodies of water is mainly vertical, although the resultant vertical flow may ze zero. This phenomenon of the interchange of small masses of water has been variously referred to as an alternating convective circulation, mixing phenomenon, eddy or vortex motion, and turbulence. Jacobsen's and other recent investigations in this field indicate that this phenomenon is the cause of the processes of diffusion, heat conductivity, and frictional resistance peculiar to oceanic conditions. Comparable values of the "Mischungsintensitat," a coefficient indicating the intensity of the rate of interchange of small water masses, have been deduced independently from the distribution of temperature and salinity, and also from the dynamical treatment of current observations. Thus studies of temperature and salinity distributions may yield appropriate values of the frictional resistance, an essential factor in the dynamical solution of ocean-current problems as well as certain tidal problems. This frictional resistance about which there is so little definite information appears to vary widely with the locality, wind velocity and other factors. It is not a "physical constant" of the *substance*, water.

The precise nature of this mixing motion can not be directly determined, but various reasonable assumptions regarding it can be made, and combined with known fundamental facts into a quantitative theory or generalization, from which deductions can be made, and tested by comparison with observations. Encouraging results already reached appear to justify

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[1] Ekman, V. W. 1905–06. On the influence of the earth's rotation on ocean currents. Arkiv for Matematik, Astronomi och Fysik, 2, 1-53, 1 pl. and 10 figs.

[2] Nansen, Fridtjof. 1902. The Norwegian North Polar Expedition, 1893–1896. Scientific Results, Vol. III, Longmans Green & Co. London, part IX, pages 1–427. 33 pls.

[3] Bjerknes, V. F. K. 1901. Circulation relative zu der Érde. Öfversikt af Kongl. Vet.–Akad. Förhandl., 58, 739–757.

[4] Krümmel, O. 1911. Handbuch der Ozeanogriphie (Stuttgart, Engelhorn), 2, xvi, 766 pp., 182 figs. in text.

[5] Jacobsen, J. P. 1913. Beitrag zur Hydrographie der Dänischen Gewässer. Medd. Komm. Havandersogelser (Hydrografi), 1, no. 94, pp., 14 pls., 17 figs. in text.

further efforts in this direction and point to the possibility of a satisfactory coordination of the various phenomena of conduction, diffusion, and fluid friction by means of a single mathematical theory of the mixing motion. Such an investigation, if successful, would enable one to deduce the temperature distribution in a body of water gaining heat from solar radiation of given intensity, and losing heat by evaporation. Investigations of this simplest case would thus correlate under definite physical principles all of these various thermal phenomena. Again, by so amending such results as to include the effect of a given flow or current on the distribution of temperature determined for the above simplest case, estimates of a current could be made from the difference between the undisturbed and the actual temperature distribution. This has been partly worked out and applied to the determination of the velocity of upwelling in the San Diego region. Thus the temperature disturbance can be quantitatively treated as an effect of a current, without regard to its dynamical causes. Qualitative conclusions relative to ocean currents have long ago been reached from essentially the same general idea, and this fact points to the possibility of such a quantitative theory. Such general quantitative laws of the relation between currents and temperature departures from the undisturbed state might be combined with Bjerknes's dynamical theory, and thus afford a means of deducing answers to more involved questions, such as the following: Given the distribution of solar radiation over the surface of a body of water having given boundaries, and a known initial temperature distribution, to determine the resulting temperature distribution and circulation for any later time. The results of similar determining conditions have not infrequently been either assumed or surmised in order to form a basis for more far-reaching oceanographic conclusions. But it is by the *precise* formulation and sufficiently *accurate* solution of suitable specific problems sufficiently in accord with actual conditions that general laws of oceanic phenomena can be discovered and tested. And the greater the variety of such ideal problems that we are in position to attack, the greater will be our progress in the precise and detailed study of the physics of the ocean. Also, it is to be expected that a satisfactory physical theory, especially of the mixing phenomenon, would greatly aid in the solution of certain problems of sedimentation and ocean chemistry.

One problem of ocean physics, whose simplest special case would be to deduce the vertical temperature distribution in a body of water exposed to solar radiation of approximately uniform intensity over the surface and losing heat by evaporation and conduction from its surface, has received very little attention except of a qualitative or speculative nature. Yet this problem appears to be fundamental in precise oceanographic investigations. Accordingly, the author has for some time attempted to work out a solution, and after trying and rejecting various assumptions, has reached encouraging preliminary results by using certain concepts from statistical mechanics, combined with elementary laws of heat and radiation. It is hoped that these studies will have progressed far enough for publication within a year or two. It is also the intention, after this

work on temperatures is in a more finished state, to investigate the problem of diffusion in the sea by similar methods applied first to certain of the numerous salinity determinations made by the Scripps Institution. Problems of the type mentioned in this paper form an extensive and promising field of fundamental importance in oceanography, and demand the attention of all investigators interested in promoting quantitative studies of the sea, but probably only a few will desire to engage actively in their solution.

It has formerly been necessary to make a great deal of use of such scattered data as the investigator could find as a basis for theoretical work. Much has been, and doubtless will be, accomplished in that way. And all original detailed data, as well as summaries and deductions therefrom, should be accessible in some way to investigators, even if publication is not always practicable. But such a procedure has obvious disadvantages, such as insufficient or unknown precision, incomplete data, or lack of significant factors that may impair or greatly restrict the conclusions. Therefore it is also necessary to conduct special programs of observations, designed with reference to particular problems, in order to improve and supplement the above more extensive and preliminary type of work. Thus selection of the locality, season, etc., and the observation of all relevant phenomena affords as nearly as possible a realization of the advantages of the physicist who controls the conditions affecting his laboratory experiments. For example, serial temperatures observed in the central part of a high-pressure area away from currents or land, and where prevailing great depths provide results corresponding to the simplest conditions, would be of great aid in the study of "normal" temperature gradients. Such observations should also be accompanied by observations on the intensity of solar radiation, turbidity, salinity, and evaporation, and should be continued through different seasons and times of day in order to provide the most important kinds of data. Very little of this intensive type of work, carried on with sufficient continuity and completeness, has been done, and it has been restricted to certain portions of small inland seas or inshore regions. Although results thus obtained are valuable in themselves and as a means of interpreting such fragmentary and widespread data as may be available, they can not take the place of similar intensive work at selected stations throughout the ocean. Actual conditions in typical areas of the great ocean must be carefully observed and studied, if any reasonable approach to exhaustive oceanographic investigations is to be realized.

In this paper I have dwelt especially on the deductive treatment of ocean problems, because of the great need of improvement in this aspect of the subject. Although admitting that purely empirical or statistical methods are indispensable in assembling and coordinating various kinds of field data, it seemed desirable to urge the need of progress from such empirical results toward the goal of a complete deductive treatment carried out in accordance with known generalizations of physics.

STATE OF PROGRESS IN CONTINUOUS RECORDING OCEANOGRAPHICAL INSTRUMENTS

By Albert L. Thuras

The modern tendency in physical research is to replace indicating instruments by recording instruments wherever possible. This has been especially true in the science of meteorology where the recent advances have been brought about almost entirely by the remarkable improvements and developments in recording instruments. In the related science of physical oceanography there are practically no recording instruments now in general use, except possibly the tide-gage. If meteorology has been so greatly benefited by such instruments, surely in oceanography, where the changes in the physical properties are so much more regular and therefore more easily interpreted, great advances should be looked for through the addition or substitution of recording instruments.

Heretofore the methods of collecting physical data have been such that no complete knowledge of the physical characteristics of the particular body of water under investigation have been obtainable as the work is progressing. The procedure has been to lay out stations, as intelligently as possible along courses throughout the region of the ocean to be studied which will give the most important information. At these various stations with the use of water bottles and reversing thermometers samples and temperatures of the ocean water are obtained at various depths down as far as the investigations are carried. The thermometers are read as soon as the water bottles are drawn up and samples of the water are stored in bottles which are later chemically measured for salt content in a laboratory on shore. The several disadvantages of this method are apparent: (1) No working knowledge of the ocean water is immediately obtainable and consequently no rearrangement or addition of stations can be made from an examination of the data taken. This is very important especially where our knowledge of the ocean is limited and one wishes to explore the magnitude and extent of the surface and submarine currents. (2) It is impossible to obtain a corroboration of any data where there may be doubt as to the accuracy or reliability of single observations. (3) The data taken are usually inadequate and especially so at those stations where vertical lines of observations pass through various strata of water of different salinity, temperature and density. Curves and cross sections plotted from data taken in these regions are usually a matter of approximation and give very little information as to the mechanism of the mixing of waters of widely different properties. These observations are particularly inadequate in such regions as the southern end of the Grand Banks of Newfoundland where the cold waters of the Labrador Current merge into the warm saline waters of the Gulf Stream.

With the object of improving the technique of the science of physical oceanography, an effort has been made in recent years to develop practical

recording instruments which are sufficiently rugged and simple to be used on shipboard. The most important physical properties of the sea of which a continuous record should be made, are temperature, salinity, density, velocity and direction. The first three properties are so related that any one can easily be deduced from a measurement of the other two. The properties most easily measured are temperature and salinity. Salinity is defined as the number of grams of salts in a liter of sea water. From a consideration of the properties of sea water that vary with the salinity, the electrical conductivity seems to be the most susceptible to continuous measurement if the difficulty due to the variation of conductivity with temperature can be overcome. Such a method consists in measuring the ratio of the resistance of sea water in two equal or nearly equal electrolytic cells, one cell containing sea water of a known salinity and the other having the sea water to be measured flowing through it. The ratio is obtained by a Wheatstone bridge, using alternating current to eliminate polarization effects in the cells. A record of the resistance ratios of the two cells is made by an automatic electrical recorder. By immersing the two cells in the same temperature bath almost complete compensation of temperature changes is effected.

A continuous record of the temperature of the ocean is most easily obtained with a platinum resistance thermometer and an automatic regulating Wheatstone bridge quite similar to the continuous salinity recorder. This instrument has been used successfully on shipboard for several years in the region of the Grand Banks of Newfoundland and some very interesting records have been obtained which show the distribution of temperature and thereby indicate the location of ocean currents and also give a knowledge of their boundary conditions which could hardly be obtained by repeated single measurements of temperature.

The continuous recording instruments for temperature and salinity described above give only surface measurements but they could easily be constructed for making measurements below the surface. This would require the use of an insulated cable of 4 or 5 conductors which would be sufficiently strong and flexible. During 1918–19 in connection with submarine listening experiments there were constructed reinforced cables similar to these, which could be repeatedly wound on to and off of a drum and would withstand a weight of 400 to 500 pounds. Data from these instruments gave accurate curves of temperature, salinity and density from the surface down to a depth of probably 500 meters, which is the most interesting part of the ocean dynamically.

R. A. Daly of Harvard University has developed and built a thermograph which can be anchored in very deep water and will give a record of temperature for a period of several days. The instrument has an intermittent mechanism which gives periodic photographs of a mercury column. This instrument was specially designed to withstand very high pressures and it should be especially valuable in studying the small variations of temperature at great depths in the ocean.

The measurement of the movement of waters in the ocean has been quite difficult to perform experimentally. This difficulty has been due chiefly to the non-continuity of measurements and the unknown movements of the vessel from which the measurements are made. Dr. Hans Pettersson of Goteborg, Sweden, has solved this problem by his photographic current meter which will give a continuous record of both direction and velocity for a period of two weeks. This instrument with the use of special anchors and buoys, can be firmly secured at any depth down to 300 meters. The difficult problem which had to be solved in this instrument was the transfer of the motions of a propeller through a water tight case containing the recording apparatus without the addition of friction. This was accomplished by a magnetic coupling.

Dr. Pettersson has also developed densimeters to be used from shore stations which give a record of the movements of the waters of various salinities into and out of the Swedish Fjords. These instruments consist of vessels or cans whose density is equal to the average density of the Fjord waters. As the submarine waves of high salinity come in from the ocean these vessels will rise and a record of their height is automatically recorded. Some interesting theories of submarine waves have resulted from this work and the correlation between the variations of salinity and the abundance of fish in these Fjords is being studied.

This briefly describes the recent developments in recording instruments and in conclusion I wish to suggest the possible application of these instruments to future research in physical oceanography.

A comparison of the yearly observations in the region of the Grand Banks of Newfoundland shows that the volume and strength of the Labrador Current have a decided influence on the course of the Gulf Stream in that vicinity. In some years the Gulf Stream was found almost up to the southern end of the Grand Banks and in other years as far south as the 40th degree of north latitude, a variation of over 100 miles. An accurate knowledge of the volume, velocity and location of these currents from time to time and correlation with meteorological conditions might yield results of great interest.

This information could be obtained by the use of recording instruments in the straits of Florida and across the Gulf Stream before it branches out east of the Grand Banks of Newfoundland.

With continuous salinity and temperature recorders placed on trans-Atlantic vessels a complete record of the variation of temperature salinity and density could be secured across the Atlantic from month to month. These instruments would make measurements at a constant depth below the surface and might throw considerable light on the hydrodynamics of this part of the Atlantic.

It seems to me that the science of physical oceanography has passed the period of exploration and has now reached that stage in its development which calls for a program of research on a large scale with most carefully thought out plans of systematic investigation extended over long periods

of time. Results from such an undertaking I believe can be most successfully accomplished by the use of recording instruments.

Western Electric Company,
New York City.

PRESENT STATUS OF RESEARCHES ON MARINE SEDIMENTS IN THE UNITED STATES

By Thomas Wayland Vaughan[1]

INTRODUCTION

The ocean of today stands at the end of a succession of oceans that have existed since land and water were first divided from each other on the earth's surface. This fact, admitted by everyone, needs to be emphasized in order to make clear the transcendent importance of the study of marine sediments. It is possible to measure the depth and the temperature of the waters of the present ocean, to sample its waters from the surface to the bottom of its greatest abysses and examine them chemically, and to measure directly or to infer from measurable factors its currents. It is also possible to study the sediments deposited on the floor of the ocean and around its margins. These and other features of the present ocean can be known by direct processes but over a large part of the earth's surface where there was once sea there is now only land, and the depth, temperature, chemical composition, and currents of bodies of waters no longer existent cannot be measured. That seas once extended over regions now land is known through the record made by the sediments and these sediments supply the fundamental data for recognizing the physical features of the vanished oceans.

Considerable information has already been acquired on modern marine deposits and preliminary maps of parts of the ocean floor have been made. Among the sources of this information are the studies of Bailey and Pourtalès, the classic work of Murray and Renard, Murray, and Murray and Lee, Murray and Philippi, and Philippi, the many papers by Thoulet, several papers by Böggild, including his recently published "Meeresgrundproben der Siboga-Expedition," papers by Walther, and the studies of the shoal-water deposits of Florida and the Bahamas and Murray Island, Australia, with which I have been associated.[2] Of course there are many other authors but I have given the names of those who have done most in areally mapping deposits on the bottom of the sea. The great leaders are Murray and his associates, among whom Philippi is to be reckoned, and Thoulet. It is believed that the characteristics of some deposits and the relations of these deposits to the conditions under which they formed have

[1] Published by permission of the Director of the U. S. Geological Survey.

[2] K. Andrée is the author of a useful bibliography on literature on marine sediments published between 1841 and 1911. See his article, Uber Sedimentbildung am Meeresboden, Literaturzeichniss: Geolog. Rundschau, 3, 1912, 324-338.

been ascertained with enough accuracy to admit their use in interpreting geological history; but how inadequately some relations are understood is exemplified by the presence of red clay at comparatively shallow depths, 4000 meters, in the enclosed deep basins of the East Indian Archipelago. Böggild says it is necessary to conclude that the capacity of the water to dissolve calcium carbonate is greater in the enclosed basins of the East Indian Archipelago than in the open ocean.[1] Although considerable is known about marine sediments, the information is far below what is needed to understand many important features of sediments in the modern oceans and to supply a basis for interpreting ancient sediments.

RESEARCHES ON MARINE SEDIMENTS IN AMERICA

There is under the Division of Geology and Geography of the National Research Council of the United States a Committee on Sedimentation composed of fourteen members, of which I have the honor to be chairman. This committee is divided into seven subcommittees, as follows: universities and colleges east of the Allegheny Front; universities and colleges between the Allegheny Front and the Rocky Mountains; universities and colleges west of the Rocky Mountains; state geological surveys; chemical and physical researches on sediments; field description of sediments; preparation of a treatise on sedimentation. The report of the committee for the year ending on April 28 has been transmitted to the chairman of the Division of Geology and Geography and is available in mimeographed form to interested persons.

No attempt will be made to give an account of the work of the committee, as that would consume too much time, but it will be said that its scope includes both modern and ancient sediments and both continental and marine deposits. One of the purposes of the committee is to ascertain and to follow all current investigations on sediments within the United States and the machinery for accomplishing this purpose is good.

The U. S. Bureau of Fisheries is trying to arrange for a study of the sediments of the Bay of Maine but the plans have not yet been completed; and an attempt is being made to have the sediments of Chesapeake Bay studied cooperatively by the Bureau of Fisheries, the U. S. Geological Survey, and Johns Hopkins University, but the actual work on the bottom samples has not begun. Prof. G. D. Louderback has been trying to study the bottom deposits collected principally by the Bureau of Fisheries in San Francisco Bay. Some preliminary information on the samples has been published but the investigation has progressed slowly. Two researches with which I have been concerned have had grave difficulties. One of them, the study of the sediments off the mouth of Mississippi River, as representing an area in which great quantities of terrigenous material are being deposited, has come to a standstill with the resignation of Mr. E. W. Shaw from the U. S. Geological Survey. The other study

[1] Böggild, O. B., Meeresgrundproben der Siboga Expedition: Siboga Expeditie, Mon. 45, p. 11, 1916.

is on the shoal-water deposits of southern Florida and the Bahamas, as representing areas in which very little or no terrigenous material is being deposited, except at the north end of the Florida reef. Fortunately several papers on the Floridian and Bahamian samples themselves and on correlated phenomena, such as papers by Dole and Chambers and Wells on the chemistry of the waters, bacteriological studies by Drew and Kellerman, and temperature records by me, have been published, but a large quantity of data remains unpublished. I am hopeful that within a relatively few months all data already acquired, which include Ekman current meter measurements at about 15 stations, may be prepared for printing.

The researches of F. W. Clarke and W. C. Wheeler on the inorganic constituents of the skeletons of marine organisms is of prime importance but such work needs to be correlated with studies on the bottom deposits themselves. The research on which Clarke is now engaged, the composition of river water discharged into the sea, is also of great value. Wells's researches, such as his published "New determinations of carbon dioxide in water of the Gulf of Mexico" and the studies he is now making on the waters of Chesapeake Bay, are also of much value in understanding problems of sedimentation, but the sediments themselves need to be studied. Richard Field of Brown University is studying some features of modern shoal-water limestones, and E. M. Kindle of the Canadian Geological Survey is conducting important researches on modern limestones; but Kindle may not be credited to the United States.

Of the researches above enumerated, five deal with bottom samples and areal surveys of the sea bottom. The areas are the Bay of Maine, Chesapeake Bay, southern Florida and the Bahamas, the mouth of Mississippi River, and San Francisco Bay. No one of these researches is progressing in a satisfactory way. Furthermore, all these researches deal primarily with shoal-water deposits—there is no work on deep-sea deposits. The only modern deep-sea samples recently described from America are two I described in 1917 from the Tongue of the Ocean, Bahamas. This, it seems to me, is a very poor showing for the United States.

FACTORS THAT RETARD RESEARCHES ON SEDIMENTS

During the period that the United States were participants in the World War, investigations on sediments suffered as did many other kinds of scientific work and our country has not yet finished its readjustment after the conflict. Besides the interruption caused by the war, several competent investigators have been diverted by other duties and a new crop of investigators has not yet ripened.

The interruption of investigations and the diversion of investigators are not the only difficulties in the way of studies of sediments. The subject is one that does not belong exclusively in any one of the sciences as the sciences are currently classified, although those engaged in several kinds of scientific endeavor recognize the value of knowledge of certain aspects of it in the proper performance of some of their work. The engineer,

for instance, wishes to understand shore-drift in certain places and the rate of the deposition of sediment in harbors; the student of fisheries wishes to know the relations between bottom material and organisms that may be used as food; the navigator may keep his course through fog and snow by detailed knowledge of the bottom; and the geologist may utilize knowledge of sediments in interpreting some geological formation of economic significance. Of the different kinds of scientific men the geologist is the most broadly concerned, because only by an adequate knowledge of the modern can he understand the ancient deposits and it is part of his work to study the mechanical features and the constituents of sediments, both modern and ancient, though he usually feels that his attention should be directed to past rather than to present history. This is a practical day and students inquire how they can make work on sediments pay. It has been possible to provide funds for some work on sediments but the remuneration is far below that offered by oil companies.

MEANS FOR PROSECUTING RESEARCHES ON SEDIMENTS

In remarks already made I have tried to bring to your attention the present status of researches on marine sediments in this country and I have indicated some factors that I believe retard such investigations. How can the backward condition of researches in this important subject be remedied? I will venture a few suggestions.

My first suggestion is that those interested endeavor to impress upon students the scientific importance of investigations on sediments. This may best be done by the establishment of courses in sedimentation in our universities and the offering of fellowships to graduate students for investigations in the subject. At present courses in sedimentation are given at the universities of Wisconsin, Iowa, and California, and at the University of Iowa a research fellowship is maintained. Courses should be given at more universities and there should be more research fellowships. The Geophysical Union might combine with the divisions of Geology and Geography and of Biology and Agriculture and endeavor to establish two or three more fellowships in sedimentation.

In addition to university work of the kind indicated an institution or institutions in which complicated special studies may be undertaken are needed. An institution comparable to the Geophysical Laboratory of the Carnegie Institution would fulfil the great need but an endowment that will yield an income between $50,000 and $75,000 per year is not easily obtained. However, it is worth striving for. As such an institution does not exist it may be preferable to try to utilize existing institutions by appealing to them and trying to strengthen them. The study of sediments is a fundamental of geology and the U. S. Geological Survey has recognized this and has tried to develop researches on sediments as a part of its work. Furthermore, many geologists, because of their training, are prepared to undertake such investigations. It is, therefore, suggested that those interested in such work make their desires known to the director of

the U. S. Geological Survey, that it be pointed out to him how the Geological Survey by doing such work can help science and serve other governmental bureaus, and that he be requested to do as much as the circumstances of the Survey will permit. The Geological Survey has already done enough to place students of sediments under deep obligations to it. If it could study and prepare reports on bottom specimens one of the present difficulties in the way of advance in knowledge of marine bottom deposits would be removed.

Until now it has been possible to obtain larger collections of properly taken bottom samples than it has been possible to study. The U. S. Bureau of Fisheries is fully equipped to collect samples precisely as they should be taken and the heads of that Bureau are anxious to do all they can to aid researches on sediments. Perhaps if provisions could be made to study the samples, the U. S. Coast and Geodetic Survey might make systematic collections. Other than governmental agencies, especially the Department of Marine Biology of the Carnegie Institution, have shown willingness to help in procuring bottom samples for study. The material available for investigation is large in quantity and much of it has been properly collected and is accompanied by all needed data. If these collections could be properly studied and reports on them published, what fine contributions would be made to our knowledge of marine sediments!

CONCLUSION

In conclusion I wish to emphasize the value to science of a proper understanding of the marine sediments in the ocean of today. A proper understanding of these sediments includes knowledge of the depth, temperature, and salinity of the waters above them, the distance from shore to where they were deposited, their relations to currents, and if near land, the relief of the land, its climate, and the rocks composing it. Through such knowledge of modern sediments the criteria for interpreting the sediments of ancient seas are discovered. Having established the needed criteria, the boundaries of the old seas may be traced; the physiography, constitution, and climate of the neighboring lands may be recognized, and the depth, temperature, chemical composition, and currents of the waters of the ancient oceans and the organisms that inhabited them may become known. Modern sediments, though important in understanding what is today, are doubly important because knowledge of them supplies the only key to what would otherwise be an unknown past.

THE INTERVALS THAT SHOULD OBTAIN BETWEEN DEEP-SEA SOUNDINGS TO DISCLOSE THE OROGRAPHY OF THE OCEAN BASINS

By G. W. Littlehales

The intervals between sounding stations must be gauged by the dimensions of the orographical features whose presence it is intended to disclose. Leaving out of consideration details of topography and confining the attention to features of the greatest prominence, inquiry must be made as to the form and dimensions of the slenderest isolated submarine peak that could be raised from the floor of the ocean to a mountainous height and remain standing under the stresses of its own weight and of the superincumbent body of water. For if the spacing of soundings be such as to give indication of the presence of the slenderest form that could stand, then evidence of the presence of any orographical forms that may exist is likely to be afforded. Theoretically, the shape of an isolated submarine peak would be that of a solid of revolution in which the resistance to crushing of any horizontal section is equal to the combined weight of the portion of the formation above that section and of the superincumbent body of water.

Let y denote the radius of any horizontal section and z its distance from the top of the formation. Let K denote the coefficient of resistance to crushing of the material composing the formation; w, the weight of a unit of its volume; and w′, the weight of a unit volume of sea water.

Accordingly, $\pi w \int y^2 dz =$ the weight of the formation above any section whose distance from the top is z, $2\pi w' \int y.z.dy - \pi w' \int y^2 dz =$ the weight of the water upon the formation above any section whose distance from the top is z, assuming the top of the formation to reach to the surface, $\pi K y^2 =$ the strength of any section to resist crushing, and

$$\pi w \int y^2 dz + 2\pi w' \int y.z.dy - \pi w' \int y^2 dz = \pi K y^2 \qquad (1)$$

By differentiation, equation (1) becomes

$$\pi w y^2 dz + 2\pi w' y.z.dy - \pi w' y^2.dz = 2\pi K.y.dy \qquad (2)$$

which expresses the condition that the increase of strength of any section in excess of that of the section next above is equal to the sum of the increases of the weight of the formation and the weight of the water upon any section in excess of their combined weight imposed upon the section next above.

Letting S denote the area of any horizontal section whose radius is y, and dS, the differential of S, equation (2) may be written in the following forms:

$$w.S.dz + w'.z.dS - w'S.dz = K.dS$$

$$(w - w')\,S.dz = (K - w'z)\,dS$$

$$\frac{dS}{S} = (w - w')\frac{dz}{K - w'z} = \frac{w - w'}{w'} \cdot \frac{dz}{\frac{K}{w'} - z}$$

By integration, equation (3) becomes

$$\log S = -\frac{w - w'}{w'} \log\left(\frac{K}{w'} - z\right) + C$$

in which C is the constant of integration.

$$\text{Hence } \log\left(\frac{K}{w'} - z\right) = \frac{w'}{w - w'}\,C - \frac{w'}{w - w'} \log S$$

$$\text{or } \frac{K}{w'} - z = \frac{E^{\frac{w'}{w - w'}\,C}}{E\left(\frac{w'}{w - w'} \log_e S\right)}$$

In the absence of knowledge of the value that should be assigned to K, the coefficient of resistance to crushing, this equation has been used in the generalized form,

$$A - z = \frac{B}{E\left(\frac{w'}{w - w'} \log_e S\right)} = \frac{B}{E\left(\frac{1.03}{2.67 - 1.03} \log_e S\right)} = \frac{B}{E^{1.46 \log_{10} S}},$$

to find the equation of their average form from the observed bathymetrical data on Seine Bank in latitude 33° 50′ N. and longitude 14° 20′ W., Cocos or Keeling Island in latitude 12° 06′ S. and longitude 96° 53′ E., Enderbury Island in latitude 3° 10′ S. and longitude 171° 10′ W., Funafuti Atoll in latitude 8° 25′ S. and longitude 179° 07′ E., Taviuni Bank in latitude 12° 05′ S. and longitude 174° 35′ W., and the shoal near Midway Island in the North Pacific Ocean in latitude 28° 00′ N. and longitude 177° 40′ W.

For this purpose the values of z and y, expressed in nautical miles, were inserted in the above equation, and a conditional equation was formed for each pair of coordinates relating to each of the submarine formations. From these conditional equations normal equations were

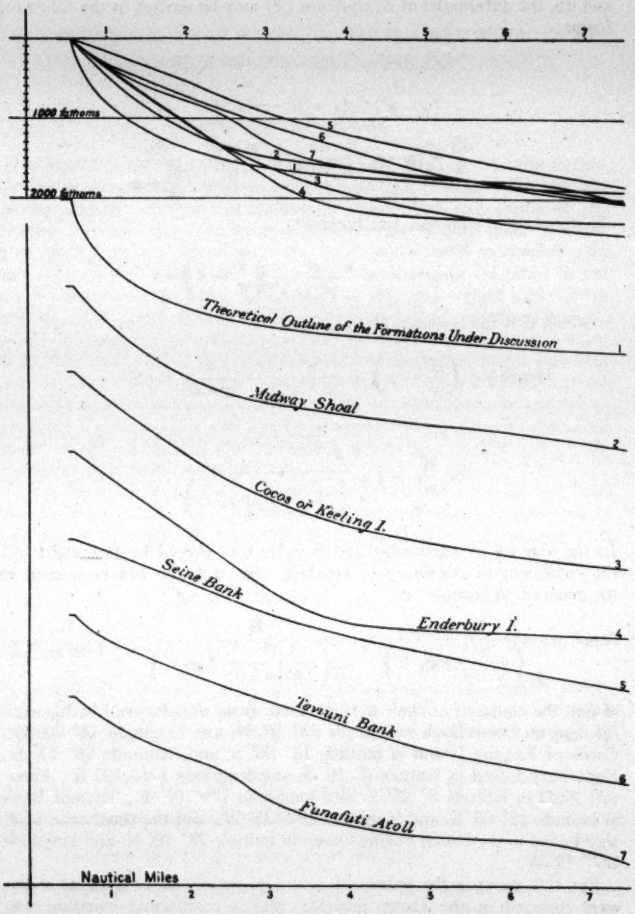

FIG. 1. Profiles of isolated submarine peaks.

found by the method of least squares, which gave the values of the constants A and B. The resulting equation is $1.87 - z = \frac{1.87}{E(1.446 \log_{10} \pi y^2)}$ and the corresponding curve, which by revolution around the vertical axis would generate the average form, is shown in figure 1, together with others which have been plotted for purposes of comparison from measured data. This investigation shows that isolated formations occupying comparatively limited areas at the bottom can and do occur in the ocean depths, and we are able to assign at once the maximum interval that should obtain between deep-sea soundings taken in operations directed toward the development of the orography of the bottom of the sea. An interval of 8 miles coupled with a differential interval of 2 miles would serve for general development, and would prove with certainty the existence or absence of any formation rising close to the surface. Of all the possible ways in which an 8-mile interval could lie with reference to a submerged peak, that which would be most advantageous for a prompt discovery of its existence is the condition in which one end of the interval is at the bottom of the slope and the other near the apex, and that which would be least advantageous is the condition in which the interval is bisected by the position of the apex. In the latter case, there would be nearly equal soundings at both ends, but the soundings at the ends of the adjacent two-mile intervals would in all probability give indication of the slopes.[1]

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[1] Following the presentation of this paper the following suggestion was made by Harry Fielding Reid:

Dr. Littlehales' remarks about the soundings in the oceans bring up a matter that I have had in mind for some time; that is, the value of a detailed sounding of a single deep. We know very little indeed about the shape or conformation of the great ocean deeps; a detailed set of soundings of a particular deep, to bring out not merely the general slope of the bottom, but also details of configuration, would be of great value. If, as seems probable, the great deeps are due to faulting, the soundings should be close enough together to show the existence of fault-scarps. A deep which offers especial facilities for such determinations is the Virgin Islands or Bronson Deep. It is a long east and west trough, lying a little north of Porto Rico, with a recorded sounding of 4,662 fathoms (the greatest depth measured in the Atlantic); although but few soundings have been made in its deeper parts. Its situation is very convenient; San Juan could be used for a base for the western part and St. Thomas for the eastern part.

There are, of course, other parts of the Carjbbean region where soundings would be valuable, but I think a detailed sounding of a single deep would yield more valuable results than scattered soundings over a larger area.

NEW METHODS OF OBSERVING WINDS AT FLYING LEVELS OVER THE OCEAN

By Alexander McAdie

Aerography may be defined in a general way as a study of the structure of the atmosphere. There are various ways of obtaining information regarding the flow of air at different levels and the conditions of density, pressure, and temperature of the mixture of air and vapor. Exploration of the upper air has been accomplished by means of close study of the clouds; the establishment of mountain observatories; the ascent of manned balloons; kites and kite balloons; sounding balloons and pilot balloons. To these we propose to add another where measurement is made from the deck of a vessel by employing certain predetermined lapse rates, or rates of fall in temperature with elevation.

The principle in brief is that provided sufficient water vapor is present and condensed as cloud, the height of the level of condensation is a function of the lapse rate. The height can be obtained then from observations of the actual temperature, the temperature of evaporation and the temperature of saturation at sea-level, making proper corrections for surface speed and direction.

It is a little more than twenty years since Teisserenc de Bort at Trappes, and Lawrence Rotch at Blue Hill, close friends and co-laborers, began the systematic sounding of the atmosphere by means of sounding balloons. With the war came a widespread use of pilot balloons. Today, *sondages* are made (or are supposed to be made) at all United States naval air stations, and at many land stations. During the war information regarding the speed and direction of the winds at flying levels thus obtained was of great value—not alone to the airmen, but also to artillerists and gas men.

There is no special difficulty in using sounding balloons or pilot balloons on land; but at sea the sounding balloon is out of the question, owing to difficulty of recovering the record. Pilot balloons, however, can be used; and during the trans-Atlantic flight of the N. C. boats, I obtained fully a hundred observations from sea-level up to 4 or 5 kilometers, while stationed on the U. S. S. *Baltimore* (mine layer).

We can not, however, expect navigating officers of our merchant marine to send up balloons, follow them with theodolites, record the elevations and angles, plot the trajectories and deduce from these the speed in meters per second and the direction of motion for the different levels. Not but that it would pay to do so; for it *will* pay any navigating officer to be posted concerning the structure of the air. It may sometimes mean the safety of the ship. And an intelligent aerographic officer with a moderate outfit of aerographic apparatus on a ship like the *Mauretania* could tell from the upper air movements studied in connection with the surface circulation, the location of the ship with reference to the true

centers of gyratory and translatory flow, and could forecast the future path of the storm. A daily weather map or one at more frequent intervals, based on reports received by radio, could be made and used to great advantage with this added knowledge of the upper air conditions.

In cloudy weather the pilot balloon may soon be lost and it is therefore advisable to substitute for the balloon method a method which makes use of clouds, especially lower clouds within 35 degrees of the zenith. Of course in dense fogs, neither method can be used.

The new method makes use of a specially stabilized nephoscope with automatic sighting rods, and an arc with tangent values; also a new type of hygroscope. The combination may be called a marine altoscope.

The nephoscope consists of a black mirror suitably mounted (for details of construction see *Blue Hill Report*, 1910) to permit of motion in azimuth, proper leveling devices, and graduated circle, reading clockwise and in either degrees and tenths or in grads. To this mirror is attached a stabilizing device, suggested by Professor R. W. Wilson of Harvard University. The mirror thus keeps a horizontal position regardless of the ship's motion.

A metal arc or quadrant springs from the plane of the mirror and is graduated in degrees, and also in natural tangents, the reason for which will appear later.

At the free end of the arc a vertical rod is mounted and carries a pantagraph or diamond-shaped rectangle supporting two rods for sighting the cloud. Use is made of the reflection of the line joining cloud and eye, and the second sighting rod forms a straight line prolongation of the line from the center of the mirror to the cloud. The value of this is in fixing the eye, whatever the ship's motion may be. When once set, the eye can be withdrawn or rested for a few seconds and then brought back to the original position without delay or uncertainty. The radials can be provided with sleeves permitting extension.

In observing, first level the instrument. Bring the zero which is also 360° or 400 grads of the horizontal or azimuth circle to the true south point. The circle is graduated clockwise and the true west will therefore be 100 if scale is in grads (90° if in ordinary units).

Since the reflection of the cloud crosses the mirror in the same direction as the cloud is moving, the reading on the azimuthal circle where the cloud image passes off the black mirror will be the direction or angle *from which the wind is blowing.*

The quadrant is now swung into position, making the same angle. With the control screw provided for the purpose of raising or lowering the sighting rods, bring the nearer sighting rod into perfect alignment with the reflection of the other sighting rod. This latter rod joins the cloud point and the center of the mirror. We have now the angular elevation of the cloud from a true horizon. When this angle is 50 grads or 45 degrees, it is plain that the distance the reflection of the cloud moves in the mirror is equal to the height of the intercept corresponding to the

height of the cloud; that is, the sine and cosine of the angle are equal, and the natural tangent is unity.

In such a case, we have only to divide the height of the cloud (to be determined later) by the number of seconds to get the rate in meters per second.

If, however, the cloud line does not make an angle of 45, we use directly the value of the tangents. The following condensed table gives these values:

Tangent	*Grads*	*Degree*	*Tangent*	*Grads*	*Degree*
.4 =	24	22	1.6 =	64	58
.5 =	29	26	1.7 =	66	59
.6 =	34	31	1.8 =	68	61
.7 =	39	35	1.9 =	69	62
.8 =	43	39	2.0 =	70	63
.9 =	47	42	2.5 =	75	68
1.0 =	50	45	3.0 =	80	72
1.1 =	53	48	4.0 =	85	76
1.2 =	55	50	5.0 =	87	79
1.3 =	58	52	6.3 =	90	81
1.4 =	60	54	11.4 =	95	85
1.5 =	62	56	∞	100	90

One has only to divide the height of the cloud by the arc reading (i. e., tangent value) to get the horizontal distance. This last divided by the number of seconds gives the speed of the cloud in meters per second. We thus have direction and speed of the air at the cloud level, provided the height of the cloud is known.

To get the height we use a special type of psychrometer (McAdie cryoscope). The improvements over the usual psychrometers are:

(1) The amount of air passing over the wet-bulb is under control; i. e., a definite value is given to the wind factor in evaporating the film of water.

(2) The method of wetting the bulb is novel. The old method of using a wick or muslin cloth, bringing a constant supply of water by capillary action, is replaced by a fine metallic mesh shaped to slide over the bulb, easily wetted and containing a known small weight of water, to be evaporated in a given time.

(3) The conversion of vapor pressure into units of force permits the use of a simple equation connecting the actual temperature, evaporation temperature, and saturation temperature.

Of the above factors, the wind velocity is of great importance and must be known definitely if the humidity records are to be regarded as reliable. It may be pointed out that even in official meteorological services at home and abroad the records of relative humidity are open to criticism on the ground of uncertain ventilation. In the best forms of sling and whirling devices no record is kept of the time and number of revolutions.

In the present instrument a definite wind velocity is automatically maintained and the beginning and ending of the movement of the air over the evaporating surface, or what is approximately the same, the movement of the wet-bulb through the air, is definite. The wetted bulb can be swung either vertically or horizontally at any desired speed from 4 to 10 meters per second. The thermometers are carried by a frame which slides on the rod and their distance from the top of the rod or axis of rotation determines the velocity of the equivalent wind. Thus at a distance of 100 centimeters (39.3 in.) the bulb when whirled will travel in one complete revolution 6.283 meters (approximately 20 ft.). It is then only necessary to know the number of rotations and the time to get the speed of the wind. An automatic counter is so connected with the handle that at the completion of every hundred revolutions an alarm bell rings. With a little practice one makes 100 swings per minute.

If desired a watch may be used and the number of seconds counted. The rate mentioned, one hundred per minute, is equivalent to a wind of 10.5 meters per second (23.5 miles per hour).[1]

Now, the rate of evaporation varies as the square root of the wind velocity. Thus the rate at 10.5 m/s is to the rate at 4 m/s as 16 to 10. The hygrometric tables in common use were based on experiments in which the speed of rotation was approximately 4.5 meters per second, although no definite statements are made and there appears to have been no special attention paid to the speed of rotation or the rate of fanning of the wet bulb. Naturally discordant results are obtained by different observers. The speed mentioned (4.5 m/s) is somewhat too low for a good circulation of air, and is indeed below the average wind value at most places. The value of 10 meters seems to be a more representative figure.

In the present instrument the pressure of the water vapor at any temperature ordinarily met with above the freezing point is expressed in units of force, and so far as known this is the first instrument employing these units for water vapor. A kilobar is that pressure which if exerted as force would give an acceleration of one centimeter per second per second to a mass of weight one kilogram. Roughly, it is the pressure given by a wind of 12 meters per second on a plane one meter square and at right angles to the wind. Thus, temperature, pressure and weight are expressed in a uniform, consistent and scientific set of units, namely, the

[1] In the sling psychrometer used by the Bureau of Mines, if we assume a speed of 100 revolutions per minute the equivalent wind would be about 2.9 m/s (6 miles per hour). There is no counting device and while a higher rate can be obtained, it is difficult to count by the eye more than 120 per minute. In the whirled psychrometer used by the Weather Bureau, the radius of rotation of the bulbs is about the same as in the Bureau of Mines instrument, but a geared handle permits of varying the rate from 175 to 260. The velocity equivalents will vary from 9 to 16 miles per hour, the rate of evaporation in the former being only 75% of that in the latter. McAdie has suggested a simple form of counter for this instrument to standardize the results and has used such a device at Blue Hill Observatory for two years.

kilobar, kilograd, kilogram. These are strictly in accord with the C. G. S. system of units.

To determine relative and absolute humidities, and the temperature of saturation, the so-called dew-point, there is used an equation given by the author in the *Physical Review,* Vol. XIII, No. 4, page 285.

$$p_s = p_e - pC \quad (t - t_e)$$

in which p_s is the pressure of the water vapor at the saturation or dew-point, p_e the pressure of evaporation—that is, the wet-bulb—p the pressure of the atmosphere expressed in kilobars, C a constant, t the temperature of the dry-bulb expressed in kilograds, and t_e the temperature of the wet-bulb.

When the wind velocity exceeds 2 m/s, pC may be written as 0.18; and for purposes of quick calculation we regard it as 20 percent without materially affecting the result.

I stop at this point to read part of a letter just received from Sir Napier Shaw. He says:

> As to the inter-relation of meteorology and oceanography, I think that humidity probably offers the most promising line of attack, if we could be quite certain of getting true humidities on board ship. I suppose that there must be a mathematical expression for the absolute humidity depending upon the air current and the eddy motion which it carries. I could imagine a very useful expedition tracing the increase in absolute humidity down the Trade Wind and ultimately to the West Indies; but it is very difficult to get humidities on board ship because the dry bulb is apt to get wet and the wet bulb to get dry; and both of them to be spoiled by spray. But he will be a great benefactor who will give us a map of the distribution of absolute humidity over the Atlantic Ocean.

Three things in the quotation are important: the suggestion of the map, the expression of belief in the humidity problem as a most promising liaison between meteorology and oceanography, and the remark about the difficulty of getting accurate humidities aboard ship.

Granted, then, that we can get these humidity values at sea with much greater precision by these new instruments, we proceed to use these values in determining the cloud heights.

The temperature of saturation can be obtained without the use of tables, which are always troublesome to use aboard ship, owing to high winds, from the cryoscope, or, if desired, from the accompanying chart (figure 2).

An example will show how this is done.

Let the dry reading be 1063 and the wet, after proper precautions, 1053. The relative humidity is at once shown by the dotted line to be 74, and the dew-point, obtained by running back to left-hand edge of chart parallel to the solid lines, 1046. If the absolute humidity is desired, one has only to follow the 1046 line horizontally to the right-end edge; and one reads 11 grams per cubic meter of space.

We will call 1046 the cloud point or temperature of condensation (heretofore called dew-point, but the new name has some advantages).

What we now want is the difference between the surface temperature

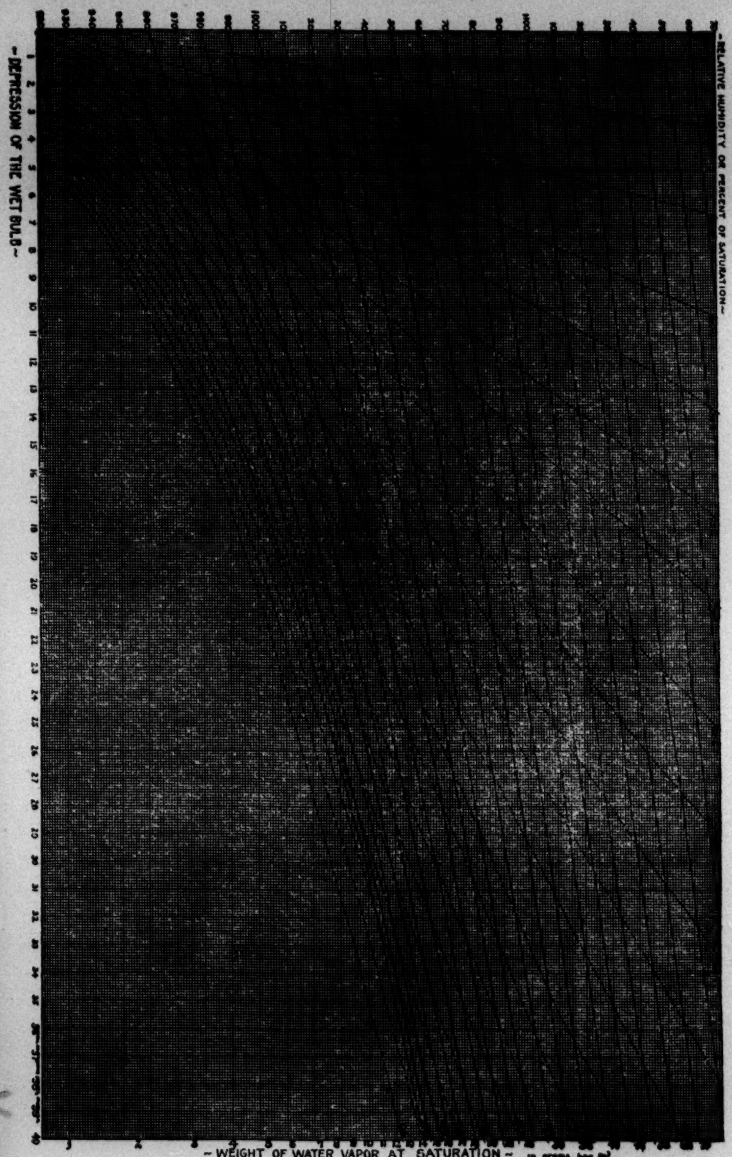
TEMPERATURE IN KILOGRADS
~ DEPRESSION OF THE WET BULB ~
~ RELATIVE HUMIDITY OR PERCENT OF SATURATION ~
~ WEIGHT OF WATER VAPOR AT SATURATION ~ in grams per m³.

FIG. 2. Absolute and relative humidity

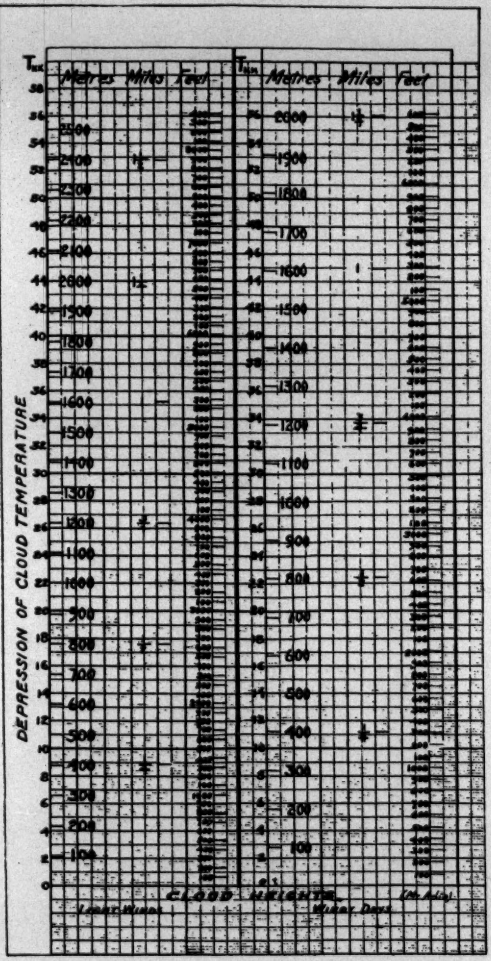

Fig. 3. Cloud heights from surface humidity

and the cloud level; or what may be called the depression of the cloud temperature (see figure 3). 1063 — 1046 = 17. The cloud height opposite 17 is 750 meters for a day of light winds and 600 meters for a windy day. A correction for percentage of saturation and type of structure is desirable.

The height of the cloud being known, the direction and velocity are obtained as described, and the observer can compare these values with the surface values. Nearly always there will be differences. In fair weather there is generally a steady shifting of the wind to a higher value for both speed and direction. At Blue Hill the mean deviation for the 1000-meter level is 7 grads, or 6 degrees to the right. The increase in speed is variable, often 100 percent in the first 500 meters, and we have instances of 200 per cent. On the land we get all sorts of structures, in some of which, such as sea breeze, the depth of the surface flow is shallow and essentially different from the flow above. The values obtained by this nephoscope-cryoscope method are approximately gradient velocities and directions. It is possible to construct a chart when gradient velocity direction and latitude are known, from which the pressure gradient can be deduced; and thus in a rough way the isolated observer could obtain the curvature of the isobar and pressure tendency. In former years this would have meant much; but now, of course, full reports can be obtained by radio and the surface isobars easily drawn.

It only remains to explain the variation in the value of the lapse rate on different days, or rather with different structures.

While the adiabate rate is 35.5 kilograds per 1000 meters, an average rate of cooling of mixed air and vapor is 21 kilograds.

For moist air saturated, a value of 18 may be taken. In windy weather, a fair value is 25 grads.

THE STEERING LINE OF HURRICANES

By Alexander McAdie

As a frontispiece to the "Manual of Meteorology," Part IV, "The Relation of Wind to the Distribution of Barometric Pressure," Sir Napier Shaw gives three storm paths of unusual duration and remarkable recurvature (see figure 4).

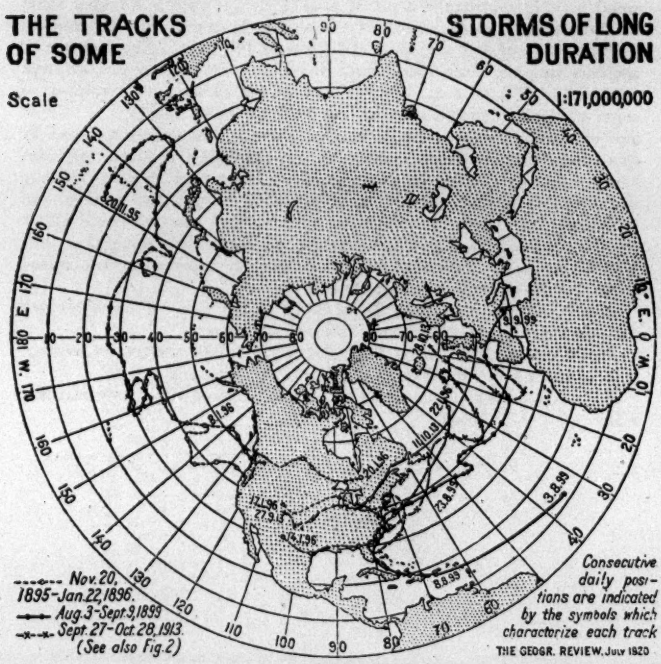
THE TRACKS OF SOME
STORMS OF LONG DURATION
Scale
1:171,000,000
Nov. 20, 1895-Jan. 22, 1896.
Aug. 3-Sept. 3, 1899
Sept. 27-Oct. 28, 1913.
(See also Fig. 2)
Consecutive daily positions are indicated by the symbols which characterize each track
THE GEOGR. REVIEW, JULY 1920

FIG. 4. The tracks of some storms of long duration (after Shaw)

Perhaps the most striking of these is a track of a typhoon or *baguio* charted by McAdie. This storm path was determined by the usual method of connecting pressure minima. The readings were obtained from abstracts of ships' logs, available through the courtesy of the Hydrographic Office. Surface winds and cloud directions were utilized as much as possible.

It was assumed that the minimum pressure and the center of circulation as indicated by surface winds were identical. It is, however, to be remembered that the wind direction as noted on the deck of a moving vessel may need correction. Furthermore the center of a cyclone is not necessarily the center of ascending air; and still further there must be in the convergence of the surface winds a certain distortion due to the travel of the storm.

The storms referred to above are perhaps best described in the words of Sir Napier Shaw (page 119).[1]

> There is evident stability in motion of this character because beginning with examples of whirls lasting for some seconds there is apparently an uninterrupted sequence by way of revolving sandstorms or dust-devils, tornadoes, or whirlwinds, to tropical revolving storms and large cyclonic areas with radii of 10 degrees or more.
>
> The only limit of the series is a revolving air-cap covering the hemisphere or a large part of it. And just as a belt of west wind or a belt of east wind may lie over these [British] Islands for weeks, so the other type of quasi-permanent atmospheric motion, which has always been thought of as a column of air in continuous revolution, may preserve its identity for days or weeks. Through the kindness of Professor McAdie of Blue Hill Observatory, Harvard University, we are enabled to give two notable examples.
>
> The first is that of a tropical revolving storm which started on a westerly track toward the Philippine Islands (where visitations of that kind are known as "Baguios"), turned round toward the north and northeast, crossed the Pacific Ocean and, after some vagaries on the North American continent, continued its journey eastward and crossed the Atlantic in the usual track of cyclonic depressions over that ocean. The whole journey lasted from 20th November, 1895, to 22d January, 1896.
>
> The second is a cyclonic depression of October, 1913, in the outer region of which the tornado was formed which caused so much destruction in South Wales on the 27th of that month.[2] The track of the main depression shows an anomalous path from Canada to the north of the British Isles. [See figures 4, 5, and 6 from the "Geographical Review."]
>
> To these notable examples has been added the long track of cyclonic depression which was figured in the Meteorological Office chart of the North Atlantic and Mediterranean for August, 1904.[3] The cyclone was first noted on 3rd August, 1899, in that part of the North Atlantic Ocean where West Indian hurricanes often take their rise. It moved westward to the West Indies, skirted the coast of Florida and turned eastward over the Gulf Stream. After some hesitation about latitude 40° W. it made for the mouth of the English Channel and, missing that, crossed to the Mediterranean, where it lost itself on 9th September, after a life of thirty-eight days.

In each of the above described storms it is evident that causes other than those developed by the rotating mass of air, operated to retard these storms in their eastward progress.

Let us now trace the path of a West Indian hurricane where the evidence is seemingly more direct.

On the morning of October 15, 1910, this storm was centered between Havana and Key West moving very slowly northward. The maximum wind velocity at the former place was 39.4 m/s (88 miles per hour); on the a. m. of the 14th; and at Key West 26.8 m/s (60 miles per hour).

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[1] See also "Wandering Storms," McArdie, *Geographical Review*, 10, no. 1, July, 1920.

[2] *Geophysical Memoirs*, no. 11. M. O. Publication, no. 22a.

[3] M. O. Publication, no. 149.

FIG. 5. Track of storm of September 27–October 28, 1913.

TRACK OF STORM
OF
MAY 8-JUNE 6, 1910
Scale 1:76,200,000
Track of main storm, May 8-27
Track of secondary storm with which it merged, May 17-27, and of combined storm, May 27-June 6
THE GEOGR. REVIEW, June 1920

FIG. 6. Track of storm of May 8–June 6, 1910.

The storm's progress northward was checked by a continental area of high pressure moving southward. Thus on the 17th we find the hurricane actually *retrograding* and centering again over Havana. As the continental anticyclone moved east, the hurricane developed a northerly component of motion and on the 18th moved across Florida. It then followed the usual hurricane track passing south of Cape Hatteras on the 20th. The hourly speed increased from 30 kms. to 50 kms. per hour and the direction of motion 40 degrees east of north.

The speed continued to increase averaging 60 kms. per hour and the direction shifted more to the east, approximately 65 degrees east of north, and so at noon of October 21 the center was in the latitude 37 degrees north and 67 degrees west.

Professor Bjerknes has remarked that "anticyclones are born as cyclones die" but the behavior of this and similar storms gives the impression that the path and speed of West Indian hurricanes, off the coast of Florida, are dependent upon the intensity and direction of advancing highs. These in turn may be but the surface expression of an advancing polar front.

Two types of south moving sub-Arctic surges which seem to control the path of hurricanes from the Caribbean Sea to the North Atlantic can be identified. The first of these is a Nichikun high. This is a more appropriate designation of what has heretofore been known as a Labrador high. According to Dr. Klotz [1] there is nowhere else in Canada "so distinct a Pamir or Roof of the World as the neighborhood of Lake Nichikun (in English, Otter Lake)." The lake itself is in latitude 53° N., longitude 71° W., and on the northwest slope of the Height of Land. The drainage is into Hudson Bay. On the south and east the drainage is into the River St. Lawrence. It is this southern slope which concerns us because south moving masses of air pass over the ridge, elevation 730 meters; and being both cold and dry and therefore heavy, fall to sea level in a comparatively short distance, 200 to 600 kilometers.

The other type of sub-Arctic surge is the "Labrador," essentially oceanic.

Both of these tongues may be portions of what Bjerknes has called the polar front. They undoubtedly play an important part in determining the speed and path of storm centers in the North Atlantic States and effectively control the path of tropical storms or hurricanes as they move from the south and change into North Atlantic cyclones.

On the Pilot Chart of the North Atlantic Ocean for October, the path of the hurricane under discussion ends abruptly in the position and on the date given above (Oct. 21). One might in consequence infer that the storm dissipated at sea.

Careful study of pressure conditions shows a depression on the 22d in latitude 35° N. and longitude 60° W. A day later it appears as one of two centers in a large depression extending from New Brunswick to

[1] In a letter to the writer.

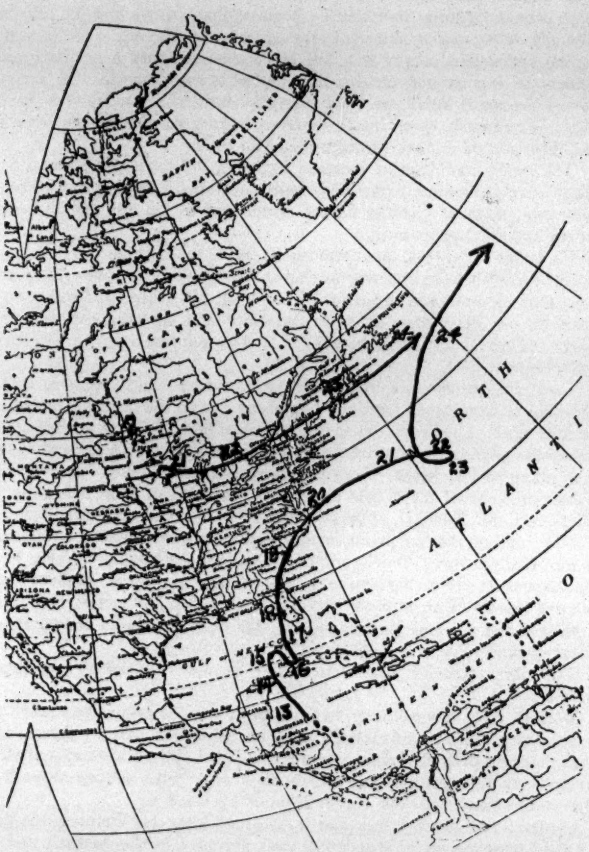

FIG. 7. Hurricane October 13–25, 1910. Lake "low" October 21–24, 1910. (Based on an equal area projection devised by B. J. S. Cahill)

Bermuda. The other center can be traced back to a storm over Lake Superior on October 21. The previous history of this depression, while somewhat obscure, is deserving of study. It appeared as an unexpected abnormal development and invalidated all forecasts made for the Lake Region, Upper Mississippi and Ohio Valleys. Where cold weather, frosts and an absence of precipitation were reasonably anticipated from an advancing high pressure (1030 kb.), there suddenly developed warmer weather with rain. On the face of the map we are unable to connect this low with a more northern slow moving depression of the Alberta type. The weather map of October 20, 1910, will repay study in connection with the steering line of cyclones.

To return to the hurricane and its further history, we have seen that when centered over Florida, there were in juxtaposition two air masses of different origin, one from the tropics with a vapor content of not less than 20 grams per unit volume (one cubic meter of space) and an average northwest speed of one kilometer per hour, while the other air mass was of sub-polar origin, approximately 20 kilograds (5.5 degrees C.) colder, and with an average vapor density of 12 grams per cubic meter. The densities of the two air masses at a pressure of 1 megabar would be approximately 1170 and 1220 grams. Air motion is initiated by difference of pressure rather than difference of density; but it is plain that the south moving air mass would continue to gain momentum and underrun the less dense northbound air. The horizontal pressure gradient was 1 kb./20 km. and hence surface velocities of 30 meters per second or higher would and did occur. The gradient velocities were 23 m/s or higher; and the radii of survature of isobars approximately 100 kilometers.

Figure 7 shows the path of the hurricane from October 13 to 25, and also the path of the lake "low" from October 21 to 24. Other charts show the surface pressure distribution on various dates.

It is much to be regretted that there are no records of winds aloft. When such data shall be available then perhaps definite relations between path, velocity and duration of hurricanes with upper winds will be forthcoming.

Recently it has been claimed by meteorologists of the Bergen (Norway) Institute that the storms of the Northern Hemisphere can be traced back to a "surface of junction of polar and equatorial air." This surface can be detected at the ground as "a line of discontinuity" in surface conditions. In other words, it is the boundary between air masses of different densities, pressures, and vapor content.

Given then a mass of warm moist air moving north of east, under the combined effects of general drift, pressure gradient and rotational deflection, and a second mass of cold dry air moving south, the surface of discontinuity should be detectable as a moving front.

Professor Bjerknes has come to the conclusion from the study of the structure of moving cyclones that a broad belt of rain accompanies the moving (and ascending) warm moist air, and a second smaller rain belt

follows, where cold dry air underruns the warm air, that is, along the wind shift or squall line.

A more important point, however, is the discovery through the use of detail maps, that the discontinuity or contrast can be traced from any cyclone to another. As expressed by Bjerknes, cyclones follow each other along a common line of discontinuity like "pearls on a string."

Furthermore this line of discontinuity surrounds the polar regions as a closed circuit. It shows how far the cold air flowing along the ground has penetrated. Shaw describes it as a kind of *polar front line.*[1]

The following substance of the discussion at the Meteorological Office on "new methods of forecasting" may make plain the leading features of Bjerknes's views.[2]

> In the case of a cyclone making progress towards the east, a sector to the south is occupied by a warm current; this warm area on the earth's surface is bounded to the north by the "steering line," to the west by the "squall-line." Bjerknes' generalization is "that these squall-lines and steering lines of all the cyclones of the northern hemisphere are parts of a single line—'the polar 'front.'" We are to think of two great streams of air, both flowing from the west, the more northerly stream being colder and carrying less moisture. The boundary between these two streams is unstable and its oscillations manifest themselves as cyclones. The warm stream overrides the cold one, which retaliates, so to speak, by turning round and kicking its partner in the back.

Charts 4, 5 and 6 are reproduced through the courtesy of the *Geographical Review,* published by the American Geographical Society, New York City.

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[1] *Nature,* January 24, 1920, p. 524.
[2] *Meteorological Magazine,* November, 1920, p. 213.

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